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Three Essays on Defending Common-Pool Resources

Lawrence Geest

THREE ESSAYS ON DEFENDING COMMON-POOL RESOURCES

A Dissertation Presented

by

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DEDICATION

To Nanny, Eros and Bill.

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It takes a village to raise a graduate student, so if you don't mind, I'd like to raise my glass to you all. The best part of graduate school are the people you meet, and besides, this might be the only bit anyone reads.

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The sun is down and I lost track of the empty glasses, but I have one last toast to make.

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Well, if you are still reading, enjoy the dissertation.

ABSTRACT

THREE ESSAYS ON DEFENDING COMMON-POOL RESOURCES

SEPTEMBER 2017

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Environmental protection often relies on cooperation between individuals in uncoordinated groups. In cases such as the management of common-pool resources, individuals must not only monitor and enforce behavior within their group to prevent over-exploitation. They must also contend with external threats on the resource like poaching. This dissertation studies cooperation to manage shared resources and deter shared threats.

The first chapter, "Deterring poaching of a common-pool resource", considers the problem of deterring a threat that cannot be perfectly observed. I present results from common pool resource experiments designed to examine the ability of a group of resource users, called insiders, to simultaneously manage their own exploitation and defend their resource from encroachment by outsiders. The insiders can use communication, peer monitoring and sanctions to coordinate their decisions. In addition,

they can sanction any outsiders they observe. I vary the insiders' ability to observe and sanction the outsiders from no observability to partial and full observability. I find a striking non-monotonicity between observability of the outsiders and levels of poaching. Poaching was higher under partial monitoring than zero monitoring, and was lower and more stable under full monitoring. Although full observability allowed the insiders to better coordinate their own harvests, they were unable to fully deter poaching because their sanctions were far too low and they were unwilling to punish low levels of poaching.

The second chapter, "Defending public goods and common-pool resources", studies cooperation and deterrence of a shared threat in different strategic environments. In many real-world social dilemmas, groups of individuals must cooperate to create surplus and defend it from theft. Theft can either foster or discourage collective action. On the one hand, a shared threat can align individual incentives. On the other hand, surplus creation may decrease if individuals are unsure how group members will contribute towards defense. Moreover, there is literature that suggests cooperation is sensitive to whether individual actions confer positive externalities (public goods, PG) or negative externalities (common-pool resources, CPR) on group members – the "cooperation divergence". To examine the relationship between cooperation and defense in different externality settings, I conduct an experiment in which a group of insiders providing a public good or conserving a common-pool resource must coordinate to deter outsiders from stealing the value of their surplus. Our theory predicts that theft will have no different effect on behavior across externality settings. However, I find that it does. Surplus creation is significantly higher in the CPR treatment, while surplus defense is significantly higher in the PG treatment. Across both treatments, I find that the shared threat increases variation within groups, but the effect is more dramatic in the PG treatment.

Finally, the third chaper, “Enforcement networks in social dilemmas”, studies how enforcement emerges and evolves in the first chapter. Sanctions can increase cooperation in social dilemmas, but they impose a high social cost until a credible threat to non-cooperative behavior is established. Moreover, credible threats depend on enforcement structure. For example, small sanctions implemented by many subjects may have a different impact on behavior than the same volume of sanctions meted out by a single subject. In order to understand how credible threats to deviant behavior emerge, it is therefore necessary to study how enforcement structure emerges and evolves in groups. I study enforcement structure by taking a network approach to data from a social dilemma experiment with peer punishment. The exchange of sanctions between subjects can be framed as a directed, weighted network that evolves, enabling us to use tools from network structure to summarize, predict and simulate behavior. I first visualize and summarize the structure of these networks and show that enforcement structure is non-random and tends to cluster around a few individuals. I then model network formation and network efficiency using an empirical framework that separately considers edge formation (a binary sanctioning event) from edge weight (sanction size) and find that subjects respond more to the act of being sanctioned rather than the volume of sanctions. Finally, I recover the underlying Markov process governing enforcement structure and simulate expected long-run behavior. I conclude with a discussion of how my approach can be used to study generalized exchange networks.

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INTRODUCTION

Fierce competition for common-pool resources (CPRs) in recent years has led to a rise in poaching (FAO et al., 2012). Poachers pose the largest threat in parts of the world where CPRs are managed by uncoordinated groups of harvesters without government help to protect the resource. A now infamous example is the case of Chilean abalone, a benthic resource considered the world's most valuable shellfish that is mainly harvested by artisanal fishers in small boats.[1] Abalone is typically eaten, but in some parts of the world it is considered an aphrodisiac and commands high bulk-prices on the black market – a recent New York *Times* investigation suggests $2,500.00 for a single abalone (*The New York Times*, 2014). Competition between poachers and fishermen in Chile led to ongoing an armed conflict – what is now called "La Guerra del Loco", The Abalone War (Gelcich et al., 2010).[2] In some cases, government intervention has made the problem worse. In South Africa, where poachers steal about seven million abalone a year worth about $440 million, the government deployed a special force to monitor the coast for poachers. Unfortunately, the government failed to monitor the special force. Corrupted force members began cooperating with poachers who sold their catch to drug cartels interested in seizing strategic coastline to push drugs into the country (Muchapondwa et al., 2014). South Africas Department of Agriculture, Forestry and Fisheries (DAFF) now believes the nation's abalone stock is on the verge of collapse.

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[1] Abalone shells are also valued due to their color and style and often trade on eBay.

[2] *Concholepas concholepas*, Chilean abalone, is colloquially referred to as "loco", a word from the Mapuche people of south-central Chile and west-central Argentina.

Poaching is a new twist on the old problem of managing common property. Aristotle may have been the first to note that the over-exploitation of shared resources stems from a misalignment between individual incentive to take and the collective incentive to preserve: "What is common to the greatest number has the least care bestowed upon it. Everyone thinks chiefly of his own, hardly at all of the common interest" (Rackham et al., 1959). The 16th century philosopher Thomas Hobbes believed the only solution was a strong central power, a "Leviathan", that coerced man to resist self-interest (Hobbes, 2006). Several hundred years later, Garrett Hardin coined the term "tragedy of the commons" and largely agreed with Hobbes: only a system that privatized common property and protected it by a central power could prevent the destruction of parks, grazing grounds, fisheries, the Earth's atmosphere (Hardin, 1968).

Twenty years later this bleak view was turned on its head by Elinor Ostrom. In her seminal book, Ostrom (1990), Ostrom cataloged case studies of communities successfully managing CPRs without the help of a central authority, from 16th century Alpine shepherds managing grazing lands to Japanese villages in the 1980's managing communal forests.[3] She also listed a litany of failures to underline her point: groups of individuals could figure out how to safeguard common property, or they could destroy it.

Ostrom's work set sail a literature spanning the social and natural sciences seeking to uncover the rules of engagements that promote cooperation among self-interested individuals in social dilemmas.[4] Starting in the 1990's the study of CPRs dove-

[3]Swiss communities seemed to manage their common grazing lands in spite of central power. After an invading Napoleon abolished the centuries-old institution in the 19th century, the grazing lands dried up.

[4]Much of this dissertation is built from this literature – see Chaudhuri (2011) for a review of experimental work. This literature led to even broader studies of how humans became a cooperative species (e.g. Bowles and Gintis (2011) and many others. While both literatures continues to grow at a pace of hundreds of papers per year, the main story is simple. Cooperation emerges and persists under stable institutions, but how institutions emerge and stabilize remains an open question.

tailed with the study of public good provision and a more general investigation of the n-person iterated prisoner's dilemma (IPD), a game theoretic model of strategic interaction featuring an equilibrium where all players are worst off (e.g. CPR destruction) and an unstable outcome where they are best off (e.g. CPR conservation).

There are many examples of the IPD but they all follow the same logic: when individuals are self-interested and possess no means to coordinate their decisions, each interaction ends up in the worst outcome. Laboratory experiments with human subjects and agent-based models, computer experiments with artificial subjects, showed that the worst outcome could be avoided when people can rely on institutions – the rules that organize social behavior – to align individual and group interests. Another way to see this is to first note that collective action problems stem from the fact that contracts–specifications for what actions should be done in every possible contingency–are incomplete (Bowles, 2009). Then, institutions serve to resolve the uncertainty of strategic interaction by providing contracts that are complete, or at least more so. Institutions impose order on social interactions, and they can be imposed top-down from a central power, or emerge bottom-up from individual decisions, group agreements, and chance.

Two institutions are studied most of all: punishment and property. Punishment means individuals often have opportunities to sanction bad or unwanted behavior at a cost to themselves, from social ostracism to pecuniary fines. The challenge is agreeing exactly what should be and should not be punished and sticking to this agreement to establish punishment as a credible threat. In other words, punishment is the institution that coordinates when to employ sanctioning technologies. In the context of CPR management, this boils down to agreements about how much sanctions should be meted out when individuals extract beyond the level that maximizes group welfare.

Instead of individuals agreeing how to share a commons, the "property-rights" approach avoids this problem by divvying up common-property into parcels of private property to each individual. The logic behind this approach is that individuals value private property more than common property and thus will take greater care to preserve their parcels without interfering in others. Or as Robert Frost once wrote, "Good fences make good neighbors." The same institution is behind the properties rights approach to the theory of the firm and to other problems of generalized team production.

Modern CPR management often features both institutions. Territorial User Rights Fisheries (TURFs) are an example (Deacon, 2012).[5] TURFs are built from the idea that central authorities are more able to allocate property than enforce it. Chilean abalone is managed by TURFs. In 1991, after the fall of the Pinochet dictatorship, a free-for-all led to the near-extinction of benthic resources, in part because the Coast Guard could not monitor the country's vast coastline. The solution was to divide the coast into zones and give the property right to groups of individuals who could then monitor and enforce each other. TURFs can be thought of as firms where the inputs to production are centrally organized. The main input to a TURF is the stock of abalone, whose health depends in part on individual harvests. Since TURF members share the input, they have an incentive to cooperate with each other to keep it in good condition.

TURFs led to a recovery in Chilean benthic resources. However, over-exploitation by group members is not the only challenge facing a TURF. As harvesters cooperate more, they produce more surplus – for example, more abalone in the water. As demand for resources like abalone exceed supply on formal markets, this surplus attracts the attention of poachers. The TURF must now exert effort monitoring and enforcing the boundary of its resource to deter poachers.

[5]Another example are Individual Transferable Quotas (ITQs) which limit how much an individual can harvest from a commons.

How poaching affects the incentives of a representative member of the TURF is unclear. For starters, deterrence is a public good, so she has an incentive to free-ride on the defense efforts by her group members. Of course, if everyone else pursues this strategy, there is no defense at all, and the poachers steal the surplus. In that case, no one will cooperate to produce surplus in the first place, leading to a return to self-interest over group-interest and ultimately a tragedy of the commons.

On the other hand, since the property rights to the resource are allocated to the group rather than to each individual, poaching poses a shared threat that could coordinate TURF members to defend the resource. Moreover, if members of the TURF have successfully worked together long enough to build a strong sense of group identity, then intergroup bias will amplify the shared threat as a coordination device, simply because members of in-groups tend to cooperate against outsiders (Cikara and Van Bavel, 2014). And yet, bad incentives that unravel it all could arise from deterrence, since deterrence leads to more surplus which then amplifies a TURF member's incentive to over-exploit the resource.

This dissertation examines how a shared threat like poaching affects the management of common property. The three essays employ a mix of techniques, including theory, experiments and computational modeling. Two themes stand out. First, I look at the effect of information, in the form of out-group monitoring, on enforcement. Second, I look at how externalities – spillover benefits or costs from individual actions – affect the creation and defense of surplus. While the focus of this work is on common-pool resources, there are other related applications involving attacker-defender situations and endogenous targets such as counter-terrorism and cybertheft.[6]

The central theme to this dissertation, and more broadly to the literature on social dilemmas, is that the success or failure of cooperation between individuals depends

[6] See for example Tambe (2011).

most of all on contracts and how they shape incentives. Technology cannot replace collective action. For example, advances in satellite monitoring of vessels are touted as the harbingers for the end of poaching. However, many poachers cannot be tracked by satellites because they remove or hack the on-board Automatic Identification System (AIS) transponder that satellites look for (McCauley et al., 2016). Moreover, the threat of poaching may only get worse. Climate change is expected to produce more natural disasters that suddenly reduce CPR stocks and lead to even fiercer competition for resources (Flato et al., 2013; Vörösmarty et al., 2000; Reuveny, 2007). It is therefore imperative to shed light on how modern schemes to manage CPRs will fare when faced with shared threats, and how they might adapt.

CHAPTER 1

DETERRING POACHING OF A COMMON-POOL RESOURCE

1.1 Introduction

Common-pool resources are prone to over-exploitation. However, observational and experimental evidence show that common pool users can often govern themselves to successfully protect their resource from exhaustion and resolve the social dilemma (Berkes 1985, Ostrom et al. 1992). Successful collective action relies on the development and enforcement of group norms. In particular, self-governance in practice and in laboratory settings often obtains from group communication and the mutual monitoring and sanctioning of group members who violate implicit or explicit norms of conservation.

However, there are many problems of common pool resources in which groups must not only regulate their own behavior but also deter outsiders from poaching. These include territorial user rights fisheries (TURFS) and the many other kinds of fishing cooperatives which exist worldwide (Deacon 2012, Ovando et al. 2013).[1] Space-based rights systems allocate fishing rights in a designated geographic area to a specific group of resource users, thus mitigating the open-access problem (Wilen et al. 2012).[2] Users are then in a better position to devise and enforce rules that

[1]Prominent examples of TURFs include those for nearshore resources in Chile, Japan (Wilen et al. 2012, Gelcich et al. 2010), Taiwan (Lin et al. 2013) and South Africa (Muchapondwa et al. 2014, Hauck and Sweijd 1999). The famous lobster gangs of Maine offer another example of common pool resource users cooperating to defend territorial boundaries (Acheson 1988).

[2]This is consistent with the first of the design principles for effective decentralized management developed by Elinor Ostrom (Ostrom 1990, McGinnis and Ostrom 1996). This principle is that the

increase the value of the resource. Crucially, this presumes that resource boundaries are effectively enforced, since successful internal coordination of harvests increases the threat of poaching. In settings in which the government is unable to provide boundary enforcement, users themselves must provide this defense. Our work is primarily motivated by the management of common pool natural resources, but there are potential applications beyond this context. Similar applications may include cooperative efforts by firms to share information and resources to protect themselves against cyber attacks (Kobayashi 2006), and neighborhood watch groups who seek to limit crime in their neighborhoods by sharing information with each other and with police (Brush et al. 2013).

The literature that uses economic experiments to investigate self-governance of common pool resources is large and active, but to our knowledge none of it is concerned with self-governance that includes protecting resources from outside encroachment. Accordingly, I have designed and conducted laboratory experiments to examine the ability of a group of resource users to simultaneously manage their own exploitation and defend the resource from poaching. Five subjects, referred to as insiders, harvest a common pool resource. They are able to communicate with each other, monitor each others' harvests freely, and sanction each other with cost. Social dilemma experiments with communication, peer monitoring and sanctioning have been studied extensively (e.g. Ostrom et al. 1992, Fehr and Gächter 2000, Cason and Gangadharan 2015, and many others). What is new with this study is the addition of a group of three subjects, referred to as outsiders, who can also harvest from the resource.

With our experiments I wish to examine how the insiders' ability to detect and sanction outsiders affects the management of the common pool resource. Thus, the treatment variable in our experiments is whether the insiders can observe and sanction

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boundaries of a resource system and the individuals with rights to harvest the resource are clearly defined.

none, all, or a subset of the outsiders. In the Zero Monitoring treatment none of the outsiders could be monitored, and thus serves as the control treatment. In the Partial Monitoring treatment, the insiders could observe the harvests of just one of the three outsiders in a round, and they could choose to sanction this individual. Finally, in the Full Monitoring treatment the insiders could observe the harvests of each of the outsiders, and they could choose to sanction any of them.

Our analysis focuses on how differences in the ability of the insiders to detect poaching affects their ability to coordinate their own harvests and to deter the outsiders. While I observe little variation in insider harvests across treatments, I present evidence that full observability of the outsiders allowed the insiders to better coordinate their harvests. However, the insiders were not able to coordinate their poaching sanctions well enough to fully deter the outsiders in the Partial and Full Monitoring treatments—their sanctions were far too low and they were unwilling to punish low levels of poaching.

Changes in the insiders' observability of outsider behavior produced significant variation in outsider harvests, including a pronounced non-monotonicity. In the Zero Monitoring treatment, in which the outsiders faced no consequences for encroachment, they showed considerable restraint by harvesting significantly less than levels implied by noncooperative Nash behavior. However, in the Partial Monitoring treatment, similar restraint early on gave way to the highest levels of poaching observed in the entire experiment. I suspect that the low, random sanctions in this treatment eroded the outsiders' willingness to restrain their harvests. By contrast, poaching was significantly less in the Full Monitoring treatment than in the other treatments. In addition, the variation in both insider and outsider harvests was significantly lower in Full Monitoring, suggesting that perfect observability allowed the insiders to exercise more consistent control over the exploitation of their resource. Overall, our results suggest that common pool resource users may often have a very difficult time deterring outside encroachment on their own, especially in settings in which the outside threat is imperfectly observed.

The remainder of the chapter proceeds as follows. In Section 2 I briefly review the experimental literature that is most closely related to our study. In Section 3 I present our experimental design, and in Section 4 I present our results. I conclude in Section 5.

1.2 Related Experimental Literature

This work builds on and adds to a large literature that uses economic experiments to examine individual behavior in social dilemmas. Most of this literature focuses on the voluntary provision of public goods, but a significant portion has examined behavior in the context of common pool resources. (See Chakravarty et al. (2011) for a detailed review of the literature.) Standard economic theory predicts that self-interested individuals will not provide efficient amounts of public goods or efficiently conserve common pool resources because of the divergence between private and group interests. In reality, however, groups have devised a myriad of institutions for collective action that help them achieve better outcomes (Ostrom 1990), and many of these institutions have been examined with laboratory experiments, especially the institutions of communication, peer monitoring and the sanctioning of noncooperative individuals.

Yamagishi (1986) appears to be the first to give subjects the opportunity to fund a system that sanctions low contributors in a public goods experiment. Ostrom et al. (1992) conducted a common pool resource experiment and found that when subjects were allowed to communicate and sanction each other they were gradually able to reduce over-harvesting. Similarly, Fehr and Gächter (2000) found that sanctioning free-riding behavior increased voluntary contributions to public goods. These results were somewhat surprising, because sanctioning in these experiments is costly enough so that strictly self-interested, payoff-maximizing individuals would not sanction others. However, later work confirmed and extended these conclusions. For example, Masclet et al. (2003) found that informal, non-monetary sanctions increased contributions over time, though they were less successful at deterring noncooperative behavior than formal, monetary sanctions. Cinyabuguma et al. (2005) found high levels of public good provision when subjects could expel members from the group, while Casari and Luini (2009) found higher cooperation levels in a public goods experiment in which sanctions were decided by consensus instead of individually. Our experiments allow for communication and peer monitoring and monetary punishment, but only by a group of insiders who are simultaneously trying to manage their harvests of a common pool resource and deter outsiders.

Other authors have explored how certain aspects of social dilemmas can diminish the effects of communication and peer monitoring and sanctions. Nikiforakis (2008) found that cooperation erodes in the presence of retaliatory sanctioning of cooperative individuals who sanction free-riders. Fowler (2005) found that second-order free-riders – individuals who abstain from sanctioning first-order free riders so as not to incur additional costs – diminish the influence of sanctioning behavior. Cason and Gangadharan (2015) revisited the nonlinear payoff structure of Ostrom et al. (1992). They noted that nonlinear payoffs push the social optimum and Nash equilibrium closer together, and hypothesize that subjects may have a harder time distinguishing pro-social from anti-social behavior in non-linear environments. Accordingly, Cason and Gangadharan (2015) found more cooperative behavior in a linear-payoff environment than in a comparable non-linear environment. Our experiments feature a non-linear environment that is similar to that of Ostrom et al. (1992) and Cason and Gangadharan (2015).

Another real aspect of social dilemmas that has been shown to complicate cooperation in the lab is that individuals often have a limited ability to observe the behavior of their peers. A number of authors have examined how imperfect peer monitoring, that is, the stochastic ability to observe other's behavior, affects voluntary public good contributions. In a public goods experiment without sanctioning Spraggon et al. (2015) found that the random public revelation of individuals' contributions led to greater contributions, but there was not a significant difference between revealing three of five individuals' contributions and revealing every individuals' contributions. In games of both imperfect monitoring and sanctioning, Carpenter et al. (2012) and Boosey and Isaac (2016) note that imperfect monitoring results from the monitoring network that governs the interactions between individuals, and thus the manner in which imperfect monitoring affects cooperation is determined by the underlying network structure. Our experiments can also be viewed as varying the network structure of a common pool resource game. Our experiments feature perfect monitoring within the inside group, the outsiders cannot monitor any other subject, and the treatment variable of interest is whether the insiders can observe the harvests of zero, one, or all of the outsiders.[3]

Given that our experiments feature two groups harvesting from the same common pool resource, the literature on the effects of group competition on cooperation may be relevant. The consistent finding from this literature is that between-group competition tends to improve within-group cooperation by pitting group members against a common opponent instead of one another (Bornstein and Ben-Yossef 1994,

[3]Other literature examines the impact of inaccurate monitoring on cooperation. For example, in the experiments of Ambrus and Greiner (2012) and Grechenig et al. (2010), subjects' public good contributions were revealed with some stochastic error about their true contributions. Ambrus and Greiner (2012) found that subjects playing a public goods game with accurate monitoring cooperated more and enjoyed higher payoffs when they had a more severe punishment technology (i.e., a 1:6 cost-to-sanction scheme instead of a 1:3 scheme that is more commonly used). Under accurate monitoring, sanctioning was concentrated early on to set a precedent of enforcement. Establishing an enforcement precedent was more difficult under inaccurate monitoring, so the benefits of increased contributions with a more severe punishment technology were offset by the costs of more frequent sanctions. These results are consistent with Grechenig et al. (2010), who also found that inaccurate monitoring led to more punishment and reduced payoffs. In our experiments, insiders can observe the harvests of the outsiders without error or not at all.

Gunnthorsdottir and Rapoport 2006, Tan and Bolle 2007, Markussen et al. 2014, Cárdenas and Mantilla 2015). In this literature, however, the benefits of increased within-group cooperation accrue exclusively to group members. This assumption fails to hold when the threat of poaching is introduced to a social dilemma, as poaching is characterized by independent outsiders who can capture the benefit of insider efforts to conserve the resource. Therefore, the conclusions of the literature on inter-group competition cannot be directly applied to our poaching problem.

In fact, Schmitt et al. (2000) is the only other published study that I am aware of that uses economic experiments to investigate the problem of outsiders encroaching on a common pool resource. In this chapter, the authors focused on the effectiveness of communication in producing more efficient harvests when communication is limited to a subset of harvesters. Their results suggest that communication may not be very effective at coordinating harvests when there are outsiders exploiting the same resource. There are two main differences between our work and Schmitt et al. (2000). First, given the intuitive findings of Schmitt et al. and the contexts I am interested in, I chose not to explore the effectiveness of communication in the presence of outsiders–the insiders in our experiments could always communicate with each other. Second, and more importantly, Schmitt et al. (2000) did not give their insiders the ability to monitor and sanction outsider harvests. In contrast, monitoring and its effects on the ability of a group of common pool resource users to deter encroachment is the main focus of our work.

1.3 Experimental design

1.3.1 The CPR environment

In our experiments, two groups, insiders and outsiders, harvest from a common pool resource. The insiders may use communication, peer monitoring and punishment to coordinate their harvests, and they may punish outsiders for harvesting the resource if they observe them. The outsiders are not able to sanction anyone, and they cannot communicate with each other or with the insiders.

1.3.1.1 Payoffs

The experimental design is an extension of Ostrom et al. (1992), who model a common pool resource exploited by a fixed number n of identical, risk neutral harvesters. Excluding the costs of sanctioning others or being sanctioned by others, a harvester h has the payoff function,

$$\pi_h = c(e - g_h) + \frac{g_h}{G}F(G), \tag{1.1}$$

where e is the harvester's endowment that can be used to harvest g_h from the resource or in another pursuit with constant marginal return c. In addition, $G = \sum_{h=1}^{n} g_h$ is aggregate harvest and $F(G) = aG - bG^2$. $F(G)$ is interpreted as the return to the group from aggregate harvests G, of which h earns a share g_h/G. Substituting $F(G)$ into (1.1) yields

$$\pi_h = c(e - g_h) + g_h(a - bG). \tag{1.2}$$

The parameters of the payoff function satisfy $a > c > b > 0$ and $0 < b < 1$.

The harvesters are divided into insiders and outsiders: there are n_i insiders and n_o outsiders, with $n_i + n_o = n$. Individual h belonging to group k harvests g_{hk}. Aggregate insider harvests are $G_i = \sum_{h=1}^{n_i} g_{hi}$ and aggregate outsider harvests are $G_o = \sum_{h=1}^{n_o} g_{ho}$. In our experiments, each subject received a fixed payment $T > 0$ to guard against bankruptcy. Making these modifications to (1.2) gives us the payoff function for a harvester h in group k,

$$\pi_{hk} = T + c(e - g_{hk}) + g_{hk}\left(a - b\left(G_i + G_o\right)\right). \tag{1.3}$$

The parameters I use in our experiments are given in Table 1.

Parameter	n	n_i	n_o	a	b	c	e	T
Value	8	5	3	31	0.4	2	12	88.8

Table 1.1: Experiment parameters

1.3.1.2 Sanctions and deterrence

As is typical in the peer monitoring and punishment literature, insiders can punish other insiders to motivate more conservative harvests within their group. In our experiments the insiders can also punish the outsiders they observe to deter them from poaching. I adopt the seminal design of Fehr and Gächter (2002) and allow insiders to sanction others on a 1:3 basis. It costs an insider one experimental dollar to reduce the payoff of another subject by 3 dollars.[4]

Insiders can always observe the harvests of other group members, but they may have a limited ability to observe and sanction the outsiders. Suppose that insiders can observe without cost the harvests of $n_m \leq n_o$ outsiders, chosen randomly. It is well known that punishment is a public good that suffers from the standard free-rider problem (Chaudhuri, 2011). Since individual insiders have little incentive to unilaterally punish others, to deter poaching they must collectively commit to a punishment threat. Suppose that the insiders commit to imposing a punishment p on any outsider they observe. Finding the punishment threat that will deter an outsider is straightforward. Outsider h's expected payoff from poaching is $\pi_{ho} - p(n_m/n_o)$, while he simply earns the value of his endowment $T + ce$ if he does not poach. The minimum punishment threat that deters the outsider from poaching is p such that $\pi_{ho} - p(n_m/n_o) \leq T + ce$, yielding

[4]The 1:3 cost-to-sanction ratio has become standard in the literature, but other authors have explored alternatives. For example, Egas and Riedl (2008) found higher levels of cooperation with smaller cost-to-sanction ratios. Nikiforakis and Normann (2008) suggest that 1:3 peer punishment may be the minimum cost-to-sanction ratio needed to sustain cooperation in laboratory public goods games.

$$\overline{p} = \frac{n_o(\pi_{ho} - (T + ce))}{n_m}.$$

This relationship implies that to deter poaching, insiders much commit to sanctions that remove the gain from any poaching they observe (i.e., $\pi_{ho} - (T + ce)$), times the reciprocal of the proportion of outsiders they can observe (n_o/n_m). For example, if the insiders are able to observe the harvests of only one of three outsiders, then they must commit to imposing a punishment that is three times the gain from the observed outsider's poaching. I should emphasize that establishing a credible threat to deter poaching may be very difficult. For all poaching the insiders observe, each of them must overcome their incentive to free-ride on the punishment choices of other group members, and they must collectively decide how to distribute the burden of punishment.

In addition, the insiders must agree on how to sanction poaching. They could, for example, impose higher sanctions on higher levels of poaching. Doing so would have the benefit of providing marginal deterrence to limit poaching even if the insiders failed to provide full deterrence to completely eliminate poaching. On the other hand, if insiders do not condition their sanctions on the level of poaching, then a particular sanction becomes a fixed component of an outsider's payoff and the threat of sanctions will either fully deter poaching or not at all. The experimental design does not suggest a particular strategy for sanctioning poaching, so the form of this strategy is an empirical matter that I address in our results.

1.3.1.3 Benchmarks

In the meantime, it is useful to derive benchmark outcomes under the assumptions that the insiders fully deter the outsiders or not at all. Given the parameters of our experiments in Table 1, I present several benchmarks in Table 2. In this table, g_i and π_i denote symmetric harvests and payoffs for the insiders, while g_o and π_o denote symmetric harvests and payoffs for the outsiders. The payoffs do not include sanctions sent or received, because sanctions do not occur in these outcomes. If the insiders fully deter the outsiders then they are never sanctioned. If the insiders fail to deter the outsiders at all, then sanctions are not worthwhile.

The first benchmark in Table 2, denoted S, is the social optimum in which the harvests of both insiders and outsiders are chosen to maximize the joint payoffs of all harvesters combined. Of course, achieving this outcome would require coordinating the harvests of all insiders and outsiders, which is not possible in our experiments. Another standard benchmark for games of this sort is the noncooperative pure-strategy Nash equilibrium, which is denoted NC/ND in Table 2. In our context, this outcome obtains when the insiders are not able to coordinate their harvests to maximize their group payoffs (signified by NC) and they cannot deter the outsiders from poaching (signified by ND). As expected, relative to the standard Nash equilibrium, the social optimum involves lower harvests and higher payoffs for both insiders and outsiders.

More interesting are the effects of insiders coordinating harvests and deterring poaching. With communication and peer monitoring and sanctions, the insiders in our experiments are given ample opportunity to coordinate their harvest decisions. The C/ND benchmark assumes that the insiders coordinate their harvests to maximize their payoffs (C), but they cannot deter the outsiders (ND). Note that by coordinating their harvests, the insiders are able to increase their payoffs by only a small amount relative to the NC/ND benchmark. As the insiders reduce their harvests in the C/ND outcome, the outsiders take advantage and poach to the 12-unit capacity. In a sense, the insiders create value by coordinating their harvests that is then captured by the outsiders. Consequently, there is little reason for the insiders to coordinate their harvests if they cannot deter the outsiders. When they can deter the outsiders so that there is no poaching, the insiders earn significantly more, even if they are unable to coordinate their harvests. Note in Table 2 that relative to the NC/ND outcome, insider payoffs are 24% higher when they deter the outsiders, but

Outcome	g_i	π_i	g_o	π_o	$n_i\pi_i + n_o\pi_o$
S	4.53	178.40	4.53	178.40	1427.20
NC/ND	8.05	138.90	8.05	138.90	1111.20
C/ND	3.65	139.45	12	200.40	1298.45
C/D	7.25	217.93	0	112.8	1161.65
NC/D	12	172.80	0	112.8	936.00

Table 1.2: Benchmark outcomes

are not able to coordinate their harvests (the NC/D outcome). Their payoffs are 57% higher when they can deter the outsiders and coordinate their harvests to maximize their joint payoffs (the C/D outcome).

While the outcome in which the insiders cannot coordinate their harvests but they do deter poaching may not seem very plausible, I mention it briefly for completeness. In this outcome, denoted NC/D in Table 2, the insiders harvest to the 12-unit capacity constraint. Note that the insiders are better off in this outcome than in either of the outcomes in which they are unable to deter poaching.

1.3.1.4 Harvest best-responses

The benchmarks in Table 2 are useful for revealing the joint values of deterring poaching and coordinating harvests, but in our experiments I find that the insiders were not able to fully deter poaching and the outsiders did not fully exploit the insiders' inability to deter them. Hence, the benchmarks in Table 2 do not allow us to judge how well the insiders were able to coordinate their harvests, given the behavior of the outsiders. Instead, I construct optimal best response functions to the outsiders' poaching when the insiders coordinate their harvests and when they do not, and then compare their actual harvests to these best responses. More explicitly, using the individual payoff function (1.3), when the insiders coordinate their harvests, given the poaching of the outsiders, they collectively choose $g_{hi} \in [0, e]$, $h = 1, ..., n_i$, to maximize their aggregate payoffs

$$\sum_{h=1}^{n_i} \pi_{hi} = \sum_{h=1}^{n_i} \left(T + c(e - g_{hi}) + g_{hi}\left(a - b\left(G_i + G_o\right)\right)\right). \tag{1.4}$$

It is straightforward to show that the symmetric cooperative best-response of the insiders to outsider poaching is

$$g_i^C(G_o) = \max\left\{\min\left(\frac{(a - c - bG_o)}{2bn_i},\, e\right),\, 0\right\}. \tag{1.5}$$

(The C superscript indicates that the insiders are able to coordinate their harvests by maximizing (1.4), given poaching by outsiders G_o). When the insiders are unable coordinate their harvests, their individual Nash strategies are determined by choosing $g_{hi} \in [0, e]$, $h = 1, ..., n_i$, to maximize

$$\max_{(g_{hi})} \ \pi_{hi} = T + c(e - g_{hi}) + g_{hi}\left(a - b\left(G_i + G_o\right)\right),\ h = 1, ..., n_i. \tag{1.6}$$

The insiders' symmetric noncooperative best-response to outsider poaching is

$$g_i^{NC}(G_o) = \max\left\{\min\left(\frac{(a - c - bG_o)}{b(n_i + 1)},\, e\right),\, 0\right\}. \tag{1.7}$$

(The NC superscript indicates that the insiders are not able to coordinate their harvests). It is straightforward to show that $g_i^C(G_o) < g_i^{NC}(G_o)$ for interior insider harvests. To judge how well the insiders coordinate their harvests, given the poaching of the outsiders, in every period of the experiment I will compare the insiders' average harvests to (1.5) and (1.7), given the experiment parameters and the actual level of poaching by the outsiders.

It is also straightforward to show that the outsiders' symmetric noncooperative best-response to the insiders' harvests is

$$g_o^{NC}(G_i) = \max\left\{\min\left(\frac{(a-c-bG_i)}{b(n_o+1)},\, e\right),\, 0\right\}. \tag{1.8}$$

(The NC superscript indicates that the outsiders do not coordinate their harvests). Comparing observed poaching in our experiments to (1.8) will give us information about the outsiders' deviations from Nash poaching behavior.

Note that the best-response functions (1.5), (1.7) and (1.8) do not include any direct information about insider sanctions on each other or on the outsiders. As I employ these best response functions to gauge how actual harvests deviate from cooperative and non-cooperative best responses, I need to keep in mind that these best-response functions are independent of what the insiders do to sanction others.

The best response functions (1.5), (1.7) and (1.8) are also useful guides in thinking about how outcomes involving imperfect deterrence of outsiders and imperfect coordination of insider harvests may differ from the benchmarks in Table 2, in particular the standard Nash equilibrium NC/ND . As is typical of common pool resource games, (1.5), (1.7) and (1.8) are downward sloping in the opponent group's harvests (for harvests between zero and the capacity constraint), indicating that insider and outsider harvests are strategic substitutes for each other. Assuming for a moment that the insiders are not able to coordinate their own harvests, imperfect deterrence would lead to lower outsider harvests, given the harvests of the insiders. This could be represented by a best-response function for the outsiders that is shifted down from (1.8). However, since insider and outsider harvests are strategic substitutes, lower poaching would motivate insiders to harvest more. Since the insiders' responses to lower outsider harvests are likely to be less than one-for-one (note that the slopes of (1.5) and (1.7) are strictly between negative one and zero), total harvests from the resource would decrease. Thus, imperfect deterrence of the outsiders is likely to result in lower outsider harvests, potentially higher insider harvests, but strictly lower harvests overall relative to the Nash equilibrium.

However, recall that greater deterrence increases the value to insiders of coordinating their own harvests. Thus, the effects of imperfect deterrence of outsiders may be more complicated if the insiders simultaneously lower their harvests because they are able to coordinate their harvests more effectively. The combined effect of greater deterrence and better coordination of insider harvests is ambiguous. Moreover, the reaction of the outsiders to lower harvests of the insiders may be to increase poaching. It seems unlikely that the insiders would allow imperfect deterrence and imperfect coordination to result in an outcome with greater poaching, but it is important to realize that the effect of imperfect deterrence on poaching can be partially offset by more effective coordination of insider harvests. While imperfect deterrence of outsiders and imperfect coordination of insider harvests have countervailing effects on both insider and outsider harvests, the effects on aggregate harvests are not ambiguous—both produce lower aggregate harvests than the Nash equilibrium.

1.3.2 Experiment procedures

Each experiment involved a group of eight subjects, who were randomly assigned to two subgroups. Group 1, the insiders, consisted of 5 subjects, while Group 2, the outsiders, consisted of three subjects. (I avoided the use of the terms insiders and outsiders in the experiments). Subjects stayed in the same group and sub-group and retained the same identification number for the duration of the experiment.[5]

Subjects participated in fifteen periods of the experiment, with each period consisting of either four or five stages. The timeline of events in each period is summarized in Figure 1.1. Stages in which the insiders were active are above the timeline,

[5]Although our experimental design is an extension of the design introduced by Ostrom et al. (1992) to include poaching, many of its features follow Cason and Gangadharan (2015). The experiment instructions attached as a reviewer's appendix.

while stages in which the outsiders were active are below the timeline. In each odd-numbered period, insiders could communicate with each other via an online chat. The insiders could not communicate with each other in even-numbered periods and outsiders could never communicate with others.[6]

In the harvest stage, subjects chose how much of their 12-unit endowment they would use to harvest from the common pool. I use neutral language and a neutral frame in our experiments. For example, I used the terminology "invest in a common account" instead of "harvest". Moreover, I did not frame the experiments as insiders having the sole property right to a resource, and thus outsider harvests were not framed as taking or stealing from the resource. An alternative design that assigns explicit property rights to the insiders might also yield important insights. [7]

Harvests generated payoffs according to (1.3) with the parameters in Table 1.2. When the harvest stage was complete, all subjects were given the results from the stage. In addition to their individual harvests and initial payoffs, each subject was given the total harvests of insiders and of outsiders, as well as aggregate payoffs (insiders and outsiders combined).

In the sanctions stage of each period, insiders chose sanctions in the form of deduction points. (I used "deductions" instead of "sanctions" in the experiments). Outsiders could never sanction others. As noted earlier, I adopted the seminal design of Fehr and Gachter (2000) to our context. In particular, I used the 1:3 punishment mechanism in which a subject pays one experimental dollar to reduce the payoff of

[6]When designing our experiments I were concerned that outsiders would be idle for too long waiting for insiders to complete their discussions. Previous work has suggested that boredom can influence decisions in experiments (Lee, 2007). On the other hand, I wanted to give insiders ample time to discuss their joint problem of managing their harvests and deterring outsiders. This trade-off between limiting potential boredom effects for the outsiders and giving the insiders sufficient discussion time led to our compromise of having the insiders communicate in every other round.

[7]I examined the chat logs and exit surveys for indications about how the insiders viewed their rights to the resource. I found no evidence to suggest that they considered the resource to be their property, or that they thought the outsiders had equal rights to the resource.

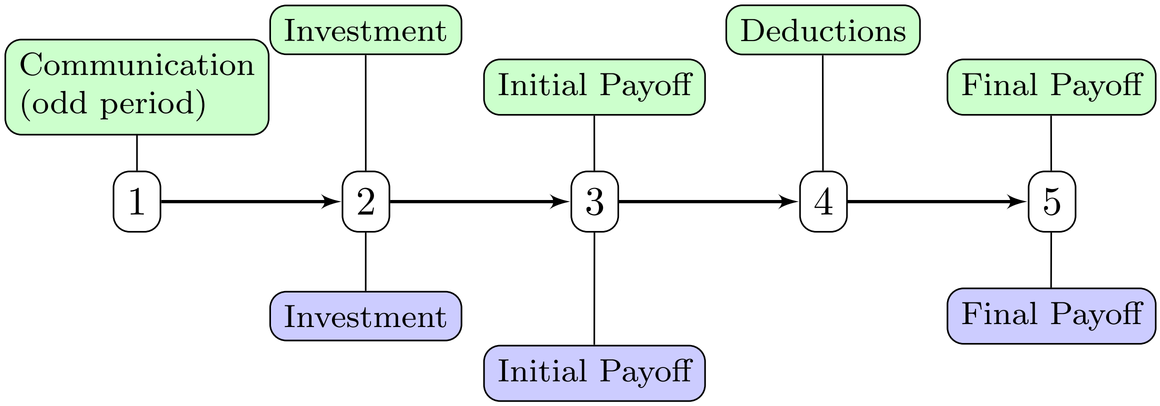

Figure 1.1: Period stages. Insiders are active above the time line, outsiders are active below the time line.

another subject by three experimental dollars. In each treatment, insiders could view the harvests of all the other insiders (along with their identification numbers) and apply sanctions. The insiders' ability to monitor and sanction the outsiders varied by treatment. An important feature of the design of Fehr and Gachter (2000) is that subjects cannot observe other subjects' sanctioning choices, even though individual investments in the common account are known. Thus, in our experiments insiders could observe each other's harvests but could not observe their punishment choices. This design element may have important consequences for their ability to coordinate their defense of the resource. When the sanctions stage was complete, subjects viewed their sanction costs, including what they spent on sanctioning others and the sanctions they received, their final payoffs for the period, and their accumulated payoff to that point in the experiment.[8]

A total of 96 subjects were recruited from the general student population at the University of Massachusetts-Amherst. Four groups of eight individuals participated in each treatment. After subjects were assigned to their groups and assigned their

[8]Only two restrictions were imposed on sanction levels. First, an insider could not choose sanctions that cost more than his or her initial payoff. Second, the final payoff of any insider or outsider could not be reduced below zero. Out of 1440 observations, payoffs were driven to zero just six times. All instances occurred in one treatment and five of them involved insiders.

identification numbers, an experimenter read the instructions aloud while the subjects followed along with their own copy. Subjects were paid \$5 for agreeing to participate and showing up on time and then earned additional money in the experiment. Subjects earned experimental dollars (\$E) that were converted to US dollars at a pre-announced exchange rate. Subjects were paid their combined earnings in cash at the end of the session. Earnings ranged between \$20.48 and \$35.29, with a mean of \$28.36 ($\sigma = 2.71$). Sessions lasted approximately 90 minutes. The experiment was conducted using z-Tree (Fischbacher 2007).

1.3.3 Treatments

Our main goal with this study is to investigate the effects of observability of outsiders on a group's ability to defend the boundaries of a common pool resource and their ability to coordinate their own harvests. Toward that end I conducted three treatments. In the Zero Monitoring treatment, insiders could not observe the decisions of any of the outsiders. In the Partial Monitoring treatment, each of the insiders could observe the harvest decision of one randomly chosen outsider and his or her group and ID numbers, and they could choose to sanction this individual. In the Full Monitoring treatment the insiders could observe the harvest decisions and group and ID numbers of all three outsiders and they could choose to sanction any of the outsiders.

One of our main questions is whether the increased ability of the insiders to observe and sanction the outsiders will lead to lower poaching. I expect that moving from Zero to Partial Monitoring will lead to less poaching. Provided that the insiders actually sanction the outsiders in the Partial Monitoring treatment, increasing the expected cost of poaching from zero should motivate the outsiders to harvest less. It also seems likely that moving from Partial to Full Monitoring will lead to less poaching as the insiders take advantage of their expanded opportunity to defend the resource.

However, I should keep in mind that greater deterrence can be partially offset by an accompanying change in the insiders' ability to coordinate their own harvests. In addition, an anonymous reviewer pointed out to us that increased observability of outsiders may also increase the difficulty of coordinating their punishment. That is, it is probably easier to coordinate sanctions for the one outsider observed in the Partial Monitoring treatment than for the three outsiders observed in the Full Monitoring treatment. It is possible that this coordination challenge is so difficult that full observability of outsiders does not lead to more effective deterrence.

If the insiders' increased opportunities to observe and sanction outsiders actually leads to less poaching, the impact on insider harvests is unclear. Insiders may increase their harvests to take advantage of lower poaching, but deterring poaching increases the value of coordinating their harvests, leading them to lower harvests. The effect of moving from Partial Monitoring to Full Monitoring on the sanctions insiders impose on the outsiders is also unclear. I may observe less punishment of outsiders in the Full Monitoring treatment if the insiders substitute increased observability for sanctions in deterring outsiders, if the coordination problem of sanctioning multiple outsiders leads the insiders to impose lower sanctions, or if less poaching by the outsiders leads the insiders to punish them less. Despite the ambiguous treatment effects concerning harvests and sanctions, if the insiders are able to use increased observability to better deter the outsiders, I expect that insiders will earn higher payoffs and outsiders will earn lower payoffs as the ability of the insiders to observe and sanction poaching increases.

1.4 Results

In this section I report the results of our experiments. I begin by examining the treatment effects on the harvest choices of both insiders and outsiders. I then examine the use of sanctions by insiders on other insiders and on outsiders. Lastly I examine how the efficiency of the subjects' choices vary across treatments.

1.4.1 Harvests

In Table 1.3, I present average harvests for both insiders and outsiders across treatments. To check for differences in harvests across time within a treatment, I also present average harvests for the first seven periods in each session and for the remaining eight periods. Throughout our analysis I take the following approaches to obtaining estimates of statistical significance. Unless stated otherwise, reported p-values are generated from linear random effects regression models that control for the dependence of individual decisions within and across time periods. I use separate regression models for insiders and outsiders, controlling for treatment, Periods 1-7 and Periods 8-15, and the interactions between treatments and the time intervals. In this section I report results from tests between coefficients.[9] In other cases, I am only interested in the treatment effects of group-level outcomes over all time periods. For these I report p-values for the nonparametric Wilcoxon rank sum test. Finally, I use Levene's nonparametric tests for the equality of variances throughout, acknowledging that this test does not account for the dependence of variances over time.

One of the central questions of this study is how the ability of insiders to monitor and sanction outsiders affects their ability to coordinate their own harvest decisions. It is clear from Table 1.3 that the insiders were never able to fully deter poaching. Moreover, as I will see, the outsiders never fully exploited the relatively low consequences of poaching imposed on them by the insiders. As noted in the previous section, to examine whether the insiders were able to coordinate their harvests while only partially deterring the outsiders, I consider the relationships between their average harvests and their symmetric cooperative and noncooperative individual best-

[9]The regression results are attached to this submission as a reviewer's appendix.

responses to the harvests of the outsiders in a period. Recall that these best-response functions are provided in equations (1.5) and (1.7). In the top panels of Figure 1.2 I display average insider harvests for each period of each treatment, along with their cooperative and noncooperative best-responses to aggregate poaching for the period. Similarly, while the outsiders do not have opportunities to coordinate their decisions, they may restrain their harvests so that they deviate from their Nash best responses. In the bottom panels of Figure 1.2 I present average outsider harvests for each period of each treatment, as well their symmetric noncooperative best-responses to the aggregate harvests of insiders for the period, given by equation (1.8).

Table 1.3 and the top panels of Figure 1.2 suggest that there is not much systematic variation in average insider harvests across treatments. Over all periods, average insider harvests are not significantly different among the treatments ($p = 0.99$ for Zero Monitoring vs. Partial Monitoring; $p = 0.41$ for Zero Monitoring vs. Full Monitoring, and $p = 0.26$ for Partial Monitoring vs. Full Monitoring). There are also no clear time trends in average insider harvests within treatments. There is a significant decrease in insider average harvests between the first half and second half of the Zero Monitoring treatment ($p = 0.00$), but it is clear in the top-left panel of Figure 1.2 that their average harvests are not monotonically decreasing over time. I do not observe a significant difference in insider average harvests in the first and second halves of the Partial Monitoring treatment ($p = 0.63$), nor do I observe a time difference in the Full Monitoring treatment ($p = 0.13$).

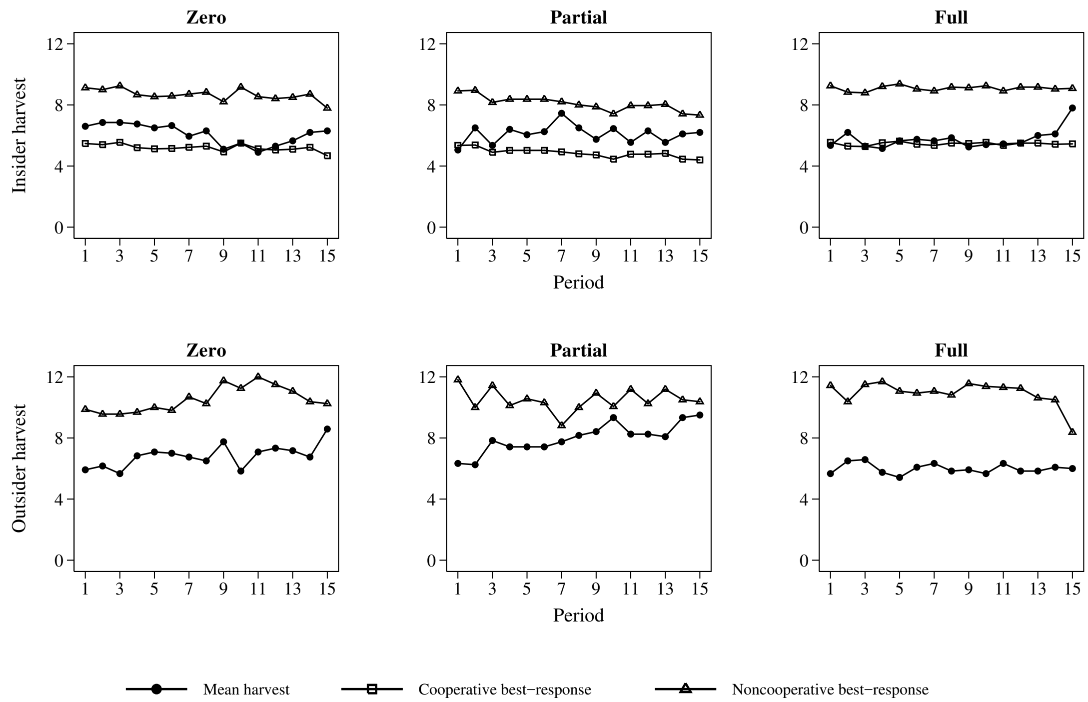

Figure 1.2: Average insider (A) and outsider (B) harvests with best-responses.

	Zero Monitoring		**Partial Monitoring**		**Full Monitoring**	
	Insiders	Outsiders	Insiders	Outsiders	Insiders	Outsiders
Periods 1-7	6.59	6.49	6.15	7.2	5.58	6.05
	(2.46)	(3.09)	(2.11)	(3.13)	(1.85)	(2.8)
Periods 8-15	5.66	7.12	6.05	8.67	5.92	5.94
	(1.64)	(3.08)	(1.49)	(2.73)	(1.47)	(2.66)
All Periods	6.09	6.83	6.1	7.98	5.76	5.99
	(2.11)	(3.09)	(1.8)	(3.01)	(1.66)	(2.72)
N	300	180	300	180	300	180

Table 1.3: Average harvest by insiders and outsiders across treatments. Standard deviations are in parentheses.

However, it would be a mistake to use the lack of large differences in average insider harvests across treatments to conclude that observability of the outsiders had little effect on the insiders' ability to coordinate their harvests. In fact, in this regard

I have some evidence that full observability of the outsiders allowed the insiders to better coordinate their harvests, despite their inability to fully deter the outsiders.

Consider the paths of average harvests, cooperative best responses and noncooperative best responses for insiders in the top panels of Figure 1.2. In the Zero Monitoring and Partial Monitoring treatments the paths of average harvests tend to be between the insiders' cooperative and noncooperative best responses. Over all periods, average insider harvests are significantly below their noncooperative best-responses in each treatment ($p = 0.00$ for each treatment, Wilcoxon rank sum test).[10] Moreover, in both the Zero Monitoring and Partial Monitoring treatments, average insider harvests are significantly higher than their cooperative best-responses ($p = 0.00$ for both treatments, Wilcoxon rank sum test). Choices in social dilemma experiments often fall between noncooperative and cooperative benchmarks, so our results for the Zero and Partial Monitoring treatments are not surprising.[11] However, note in the top-right panel of Figure 1.2 that average insider harvests in the Full Monitoring treatment track their cooperative best responses very closely, except for high harvest levels in the last period. Not surprisingly, over all periods in the Full Monitoring treatment the difference between insider average harvests and their cooperative best responses is not statistically significant ($p = 0.11$, Wilcoxon rank sum test).

I also examined differences in the variation of harvests across treatments. The variation in insider harvests is significantly higher in the Zero Monitoring treatment than in the Partial and Full Monitoring treatments ($p = 0.00$ and $p = 0.02$, re-

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[10]I calculated the best-response harvest for insiders by evaluating the insider cooperative and non-cooperative best-response functions given observed levels of aggregate outsider harvest. I then calculated average insider harvest by dividing aggregate harvest by the insiders in each group, rather than by averaging individual harvests. This approach resolves concerns about autocorrelation, allowing us to use non-parametric methods.

[11]Other studies in this literature have found that subjects in treatments without communication and peer punishment tend to harvest more than the socially optimal levels, but less than the pure-strategy Nash equilibrium levels, and communication and peer punishment tends to motivate more conservative harvests (Chaudhuri, 2011).

spectively, Levene's nonparametric test), and the variation in the Full Monitoring treatment is weakly significantly lower than in the Partial Monitoring treatment ($p = 0.10$, Levene's nonparametric test). Thus, while there are not significant differences in average insider harvest levels among the three treatments, insider harvests were more stable and they were consistent with their cooperative best-responses under the Full Monitoring treatment, suggesting that perfect observability of the outsiders allowed the insiders to better coordinate their harvests.

There is much more variation in outsiders' poaching within and across treatments. The most striking feature of outsider behavior is that there is not a monotonic relationship between the observability of their behavior and their levels of poaching.

In the Zero Monitoring treatment the outsiders did not face any consequences for poaching, yet they showed considerable restraint. Over all periods, average harvests by the outsiders in this treatment are about 3.7 units lower than their noncooperative best-responses to the insiders' harvests, and this difference is highly significant ($p = 0.00$, Wilcoxon rank sum test). There is a small upward trend in outsider poaching in this treatment, but this is largely driven by the strong end-period effect I observe in in the bottom-left panel of Figure 1.2. Moreover, the difference in average outsider harvests in the first and second halves of the Zero Monitoring treatment is not significant ($p = 0.16$).

In stark contrast, outsiders in the Partial Monitoring treatment started out with similar average poaching levels as in the Zero Monitoring treatment, but their harvests quickly rose over time. The difference in average poaching in Periods 1-7 and Periods 8-15 is highly significant ($p = 0.00$). In fact, observe in the bottom-middle panel of Figure 1.2 that average outsider harvests drift closer to their noncooperative best response paths over time. Moreover, average harvests in Periods 8-15 of the Partial Monitoring treatment are significantly higher than in the Zero Monitoring treatment ($p = 0.05$) and the Full Monitoring treatment ($p = 0.00$). The high and rising levels of poaching in the Partial Monitoring treatment cannot be explained by differences in insider harvests, because these harvests are very similar within and across the Zero Monitoring and Partial Monitoring treatments. Moreover, given similar insider harvests, I would not expect the increased observability of poaching in the Partial Monitoring treatment to lead to greater poaching, but this is what occurred.

What could explain this counter-intuitive observation? One possibility is that outsiders showed restraint in the Zero Monitoring treatment because of some sort of social preference that kept them from poaching at the level that would maximize their monetary payoffs. However, in the Partial Monitoring treatment, the random sanctions from the insiders seems to have eroded the outsiders' motivation to restrain their harvests. In addition, as I will see in the next section, the insiders did not impose sufficient sanctions to remove the financial incentive from poaching. The erosion of the outsiders'initial restraint and the low expected sanctions appears to have led them to higher poaching levels.

While the insiders were not able to fully deter the outsiders in the Full Monitoring treatment, average poaching over all time periods was significantly lower than in the Partial Monitoring treatment (p = 0.00) – average outsider harvests were 25% lower in the Full Monitoring treatment than in the Partial Monitoring treatment, and more than 30% lower in the second halves of the two treatments. In addition, there is no time trend in average levels of poaching in the Full Monitoring treatment. I find no significant difference in poaching over all time periods between the Full Monitoring and Zero Monitoring treatments (p = 0.26). However, the variation of poaching levels is significantly lower in the Full Monitoring treatment than the other treatments (p = 0.00 for both comparisons, Levene's nonparametric test).

In summary, the insiders in our experiments were not able to use imperfect monitoring and sanctioning to limit poaching; in fact, imperfect monitoring and sanctioning appears to have eroded the outsiders' willingness to restrain their harvests, leading eventually to increased poaching relative to no monitoring at all. In contrast, the insiders were able to use complete monitoring of the outsiders to keep poaching to lower and more stable levels. This in turn allowed the insiders to more effectively coordinate their own harvests.

1.4.2 Sanctions

In this section I examine how the insiders used sanctions against each other and against outsiders. I then examine the effects of sanctions on insider and outsider behavior.

Table 1.4 displays average sanctions received by insiders and outsiders across treatments and time. While there are some differences across treatments and over time for average sanctions on insiders, they tend to be very low. Moreover, note the high standard deviations in the Zero Monitoring and Partial Monitoring treatments. Like previous studies of social dilemmas with peer monitoring and punishment, sanctions are predominately zero (e.g., Cason and Gangadharan 2015, and many others); however, there are some quite extreme sanctions in the Zero and Partial Monitoring treatments. The variation in insider sanctions is significantly lower in the Full Monitoring treatment than in the Zero and Partial Monitoring treatments (p = 0.00 for both comparisons, Levene's nonparametric test for the equality of variances). This is consistent with the lower variation in insider harvests in the Full Monitoring treatment.

Table 1.4 also reveals that insiders sanctioned outsiders more heavily than themselves. Over all periods, sanctions on outsiders were significantly higher than on insiders in both the Partial Monitoring (p = 0.01) and Full Monitoring treatments (p = 0.05). Average sanctions on individual outsiders were about 50% lower in the Full Monitoring treatment than in the Partial Monitoring treatment, but this difference is not significant (p = 0.25) because of the high variation in outsider sanctions. A rough measure of expected sanctions in the Partial Monitoring treatment; that is, average sanctions multiplied by the 1/3 probability of observing an outsider, is lower than average sanctions under the Full Monitoring treatment. It appears that insiders tended to use the increased observability under Full Monitoring to increase expected poaching sanctions, but the lack of a statistically significant difference across the Partial and Full Monitoring does not allow us to make much of this result.[12]

Perhaps the most striking feature of poaching sanctions is that they were not close to being high enough to fully deter poaching. Recall that, conceptually, full deterrence requires that insiders remove all the gains from poaching multiplied by the reciprocal of the proportion of outsiders monitored. In Table 1.5 I show the average reduction in gains from poaching due to sanctions, as well as the standard deviations of these sanctions and their minimums and maximums. Deterring poaching would require poaching sanctions that were at least 300% of poaching gains in the Partial Monitoring treatment and at least 100% of poaching gains in the Full Monitoring treatment. While the maximum values in Table 1.5 indicate that there were sanctions that approached and even exceeded these levels, on average the insiders only removed about 40% of the gains from poaching in the Partial Monitoring treatment and only about 13% of the gains in the Full Monitoring treatment. It is clear that part of the reason that the insiders could not deter poaching is that they did not punish outsiders enough to make the expected sanctions from poaching exceed the gains.

[12]Interestingly, insiders' total expenditures on sanctioning were very similar in the Partial and Full Monitoring treatments. However, there was a shift toward spending more on sanctioning outsiders and less on sanctioning insiders in the Full Monitoring treatment. Relative to the Partial Monitoring treatment, expenditures on sanctioning insiders were about 65% lower in the Full Monitoring treatment ($p = 0.00$, Wilcoxon rank sum test), while expenditures on sanctioning outsiders were about 52% higher ($p = 0.00$, Wilcoxon rank sum test).

	Zero Monitoring	Partial Monitoring		Full Monitoring	
	Insiders	Insiders	Outsiders	Insiders	Outsiders
Periods 1-7	6.81	3.86	26.36	2.89	10.68
	(21.73)	(19.70)	(47.98)	(8.30)	(20.24)
Periods 8-15	1.61	8.08	31.12	1.56	18.19
	(6.68)	(42.95)	(31.87)	(6.98)	(41.89)
All Periods	4.04	6.11	28.90	2.18	14.68
	(15.81)	(34.14)	(39.91)	(1.66)	(33.66)
N	300	300	60	300	180

Table 1.4: Average sanctions received by insiders and outsiders across treatments. Non-monitored outsiders under Partial Monitoring excluded. Standard deviations are in parentheses.

	Partial Monitoring	Full Monitoring
Periods 1-7	29.86	10.45
	(56.81)	(15.19)
	[0, 194.77]	[0, 74.63]
Periods 8-15	48.45	14.90
	(56.86)	(28.07)
	[0, 284.09]	[0, 135.87]
All Periods	40.12	12.84
	(57.10)	(23.06)

Table 1.5: Average percentage reduction of poaching gains from sanctions. Standard deviations are in parentheses and minimum and maximum values are in hard brackets.

To further investigate the supply of sanctions I follow Carpenter and Matthews (2009) and Carpenter et al. (2012) and estimate the relationship between sanctions given to individuals and their harvests using a bilinear spline piecewise regression. This allows us to capture nonlinearity in the data by allowing sanctions to vary in severity before and after an endogenous kink (or knot) in the estimated regression. I estimate two separate models by pooling the data for each subject type and interacting individual harvests with treatment dummies to estimate the knot and slopes for each subject type in each treatment. The results are in Table 1.6 and graphs of the estimated regressions are in Figure 1.3.

Note first that the constants for both the insider and outsider models and the slopes before the knots for all treatments are not significantly different from zero. This suggests that the insiders tended not to punish outsiders or themselves for lower levels of harvests. Of course, deterring poaching would have required the insiders to sanction all instances of poaching, but it appears that they were willing to tolerate significant levels of poaching. For outsiders the knots for the Partial and Full Monitoring treatments are not significantly different from each other ($p = 0.43$), but the slope estimate above the knot for the Full Monitoring treatment is significantly greater than in the Partial Monitoring treatment ($p = 0.00$). Perfect observability and the insiders' willingness to sanction more egregious poachers may have led to the reduced poaching I observe in the Full Monitoring treatment relative to the Partial Monitoring treatment.[13]

In their communications with each other, the insiders promoted a number of different strategies for punishing the outsiders, but the strategy of only punishing high levels of poaching was suggested most often. For example, in the first part of the Partial Monitoring treatment, one insider suggested, "if they go above 5 punish them huge and if they stay below I let them off without a punishment" and another suggested that his or her group "keep deducting from any 2 that invests more than a few." Some subjects in the Full Monitoring treatment also promoted this strategy: "I usually just do a couple [of deduction points], just to zing anyone who gets greedy";

[13]The great majority of outsider sanctions came from insiders who also chose lower harvests. More specifically, I looked at outsider sanctions in a period by insiders who harvested below their group's mean harvest for the period and by those insiders who harvested above their group's mean. On average, insiders who harvested below their group's mean harvest imposed 80% and 85% of the sanctions on outsiders in the Partial Monitoring and Full Monitoring treatments, respectively. I also found that, on average, 75% and 62% percent of outsider sanctions in the Partial and Full Monitoring treatments, respectively, came from just two of the five insiders in a group.

	Insiders			Outsiders	
	Zero	**Partial**	**Full**	**Partial**	**Full**
Knot	7.47***	6.00***	5.85***	5.11*	6.78***
	(0.46)	(0.61)	(0.60)	(0.73)	(0.42)
Slope (before knot)	-0.01	0.05	-0.03	0.45	0.07
	(0.25)	(0.29)	(0.29)	(0.70)	(0.46)
Slope (after knot)	2.85***	1.57***	1.27***	1.67**	5.45***
	(0.46)	(0.37)	(0.39)	(0.90)	(0.73)
Constant	0.22	0.22	0.22	0.67	0.67
	(1.36)	(1.36)	(1.36)	(2.27)	(2.27)
N		899		240	
Adj. R^2		0.21		0.50	

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.6: Sanctions as a function of harvests using a bilinear spline regression. Insiders and outsiders are modeled separately. I control for individual heterogeneity (subject fixed effects) and learning (time-period fixed effects). Standard errors are clustered at the group level to control for any remaining unspecified correlation. I drop one extreme observation for insiders in the last period of the Partial Monitoring treatment.

”or give more deductions with the kid who is putting in 12”, and ”anyone goes that far, just dump deductions on them.” Another wrote in his or her exit survey, ”I wanted to discourage the individuals who invested too much. The deductions were in essence, a form of communications to Group 2. If they invested beyond the accepted range for personal benefit, I gave them deductions to remind them.”

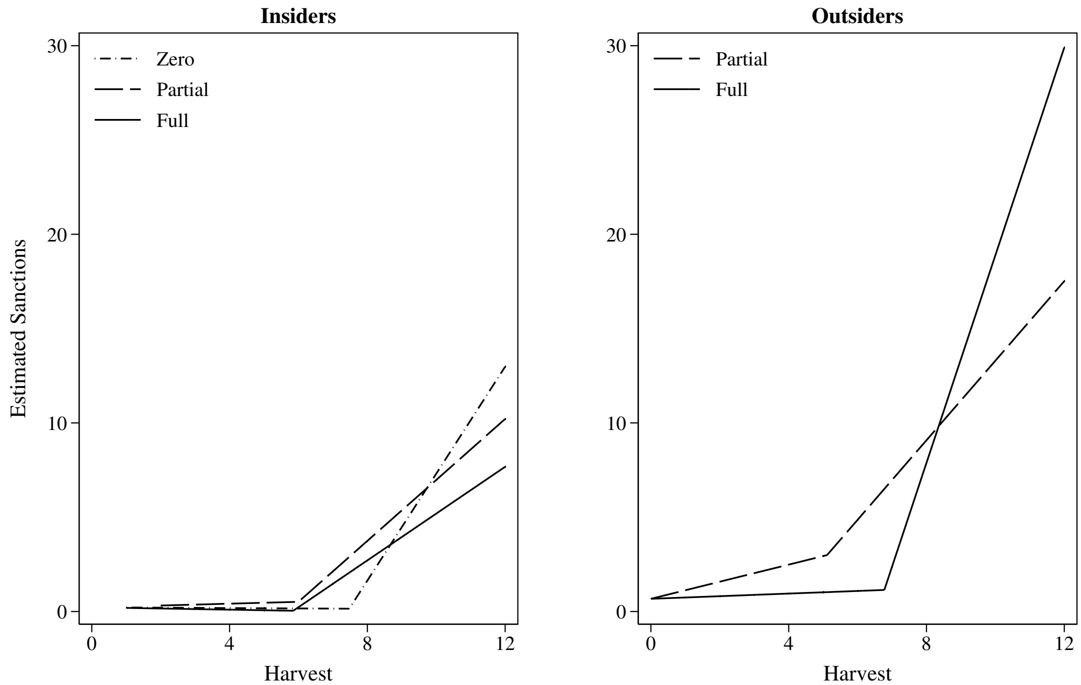

Figure 1.3: Estimated sanctions as spline functions of individual harvests.

As for the insiders' strategies to sanction other insiders, note in Table 1.6 that estimated coefficients are similar across treatments. There are some statistical differences. In particular, the knot and right-hand slope are significantly larger and steeper, respectively, in the Zero Monitoring treatment than in the Partial Monitoring treatment (p = 0.05 for the knot comparison, p = 0.01 for the right-hand slope comparison) and in the Full Monitoring treatment (p = 0.02 for the knot comparison, p = 0.00 for the right-hand slope comparison), but the differences in the knots and right-hand slopes of the Partial and Full Monitoring treatments are not significant (p = 0.85 for the knot comparison, p = 0.46 for the right-hand slope comparison). Despite these statistical differences, the strategies insiders used to sanction other insiders were very similar across treatments.

To explore how the subjects responded to sanctions, I follow the approach of Cason and Gangadharan (2015) and Masclet et al. (2003) to model changes in subjects' harvests between t and $t+1$ as functions of sanctions received in t and their deviation from mean harvests in t. I estimate separate models for insiders and outsiders, and separate models for subjects who harvested below mean harvests and above mean harvests. The results are in Table 1.7. For these results mean harvests are for both insiders and outsiders combined, but the results are robust to unique insider and outsider harvest means. Like Cason and Gangadharan (2015), I find a consistently negative and often statistically significant relationship between individual harvests and their deviation from mean harvests. This effect indicates that subjects tended to choose harvests that are similar to the rest of the group.

Controlling for the tendency toward mean harvests, I find that sanctions had mixed effects on influencing harvests. The marginal effects of sanctions on poaching are often insignificant. However, note the significant negative effect on outsiders above the harvest mean in the first half of the Partial Monitoring treatment and the strong *positive* effect for all outsiders in the second half of this treatment. It appears that random poaching sanctions led to reduced poaching by those who harvested above the mean in the first half of this treatment, but as I know total poaching increased over time in this treatment and poaching sanctions were not high enough to make poaching unprofitable. The counter-intuitive positive effect of sanctions on poaching in the second half of the Partial Monitoring treatment is consistent with our speculation that low random sanctions of outsiders eventually eroded their tendency to restrain their harvests. The same positive effect is observed for insiders in the second half of the Partial Monitoring treatment, which is harder to explain but may be consistent with the insiders' increasing inability to control harvests in this treatment. Except for the positive effect of sanctions on insider harvests in the second half of the Partial Monitoring treatment, sanctioned insiders tended to reduce their harvests and these effects are most prominent for insiders who harvested above the harvest mean.

	Insiders		Outsiders	
	Below Harvest Mean	Above Harvest Mean	Below Harvest Mean	Above Harvest Mean
Sanctions received in t				
Zero Monitoring × Periods 1-7	-0.081	-0.084		
	(0.13)	(0.05)		
Zero Monitoring × Periods 8-15	0.201	-0.073*		
	(0.15)	(0.03)		
Partial Monitoring × Periods 1-7	-0.093	-0.081*	*na*	-0.164***
	(0.28)	(0.04)		(0.02)
Partial Monitoring × Periods 8-15	0.137***	0.141*	0.375***	0.136**
	(0.00)	(0.06)	(0.03)	(0.03)
Full Monitoring × Periods 1-7	0.386	-0.103	-0.695	0.011
	(0.23)	(0.09)	(0.33)	(0.06)
Full Monitoring × Periods 8-15	-0.416*	-0.416**	-0.090	-0.040
	(0.16)	(0.13)	(0.08)	(0.04)
Deviation from the group norm in t				
Zero Monitoring × Periods 1-7	-0.306*	-0.586**		
	(0.14)	(0.15)		
Zero Monitoring × Periods 8-15	-0.990***	-0.980***		
	(0.21)	(0.15)		
Partial Monitoring × Periods 1-7	-0.362***	-0.656	-0.751	-0.557
	(0.08)	(0.30)	(0.39)	(0.52)
Partial Monitoring × Periods 8-15	-0.531**	-2.148**	-1.186**	-0.425
	(0.16)	(0.52)	(0.31)	(0.32)
Full Monitoring × Periods 1-7	-0.588*	-0.925*	-0.072	-1.171***
	(0.19)	(0.34)	(0.17)	(0.14)
Full Monitoring × Periods 8-15	-1.586***	0.037	-0.418**	-0.638*
	(0.29)	(0.34)	(0.10)	(0.23)
Constant	1.016**	1.293	1.458*	2.220
	(0.26)	(0.91)	(0.58)	(1.15)
Subject fixed effects	Yes	Yes	Yes	Yes
N	590	249	106	118
Adj. R^2	0.203	0.465	0.465	0.617

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.7: Changes in harvests for subjects who harvested above and below the group mean in period t. I control for individual heterogeneity (subject fixed effects) and learning (time-period fixed effects). Standard errors are clustered at the group level to control for any remaining unspecified correlation. For Partial Monitoring I include only outsiders who were monitored. There is no estimate for the effect of sanctions on outsiders in the first part of the Partial Monitoring treatment because no outsider whose harvest was below the mean harvest was sanctioned in the first half of this treatment.

1.4.3 Efficiency

In this subsection I examine the relative efficiency of insider and outsider choices. In each period, each subject received an initial payoff based on their harvest and the harvests of everybody else, followed by a final payoff based on the amount of sanctions received. For insiders, this also included the cost of sanctions imposed on others. I construct efficiency measures that compare initial and final payoffs to theoretical benchmarks presented in Table 2. As explained shortly, the efficiency measures are calculated differently for insiders and outsiders so they are presented separately.

1.4.3.1 Insider efficiency

Recall from Table 1.2 that insiders maximize their payoffs when they are able to coordinate their harvest to maximize their joint payoffs and they are able to deter the outsiders from poaching, that is, in the C/D outcome. The insiders are least well off in the NC/ND outcome in which they are unable to coordinate their harvests and deter the outsiders (i.e., the standard Nash equilibrium). I therefore calculate efficiency for a group of insiders in a period as the ratio of the difference between their observed payoffs and their symmetric payoffs in the NC/ND outcome to the difference between their symmetric payoffs in the C/D and NC/ND outcomes. That is, the efficiency measure for a group of insiders in a period is

$$E_{ik} = \sum_{h=1}^{n_i} \left(\frac{\pi_{ihk}^{Observed} - \pi_i^{NC/ND}}{\pi_i^{C/D} - \pi_i^{NC/ND}} \right), \; k = (initial,\; final). \tag{1.9}$$

In (1.9), $\pi_{ihk}^{Observed}$ is the observed payoff of insider h in a period (either initial payoff or final payoff indexed by k), $\pi_i^{NC/ND}$ is the symmetric individual payoff for insiders when they fail to coordinate their harvests and they fail to deter the outsiders, and $\pi_i^{C/D}$ is the symmetric payoff for insiders when they are able to coordinate their harvests to maximize their joint payoffs and they are able to deter the outsiders. Thus, E_{ik} measures a group's actual gain (or loss) relative to the Nash equilibrium as a percentage of their potential gain in a particular period. The time series for the average of this measure over groups is displayed in the top panels of Figure 1.4, and the average over groups and time are in Table 1.8. Note that final efficiency must be equal to or lower than initial efficiency, and measuring the difference between the two captures the effects of sanctions on insider welfare.

	Zero Monitoring		Partial Monitoring		Full Monitoring	
	Initial	Final	Initial	Final	Initial	Final
Periods 1-7	0.30	0.19	0.24	0.16	0.40	0.32
	(0.29)	(0.33)	(0.32)	(0.39)	(0.23)	(0.20)
Periods 8-15	0.29	0.26	0.14	0.02	0.40	0.32
	(0.20)	(0.21)	(0.19)	(0.38)	(0.14)	(0.15)
All Periods	0.30	0.23	0.19	0.08	0.40	0.32
	(0.24)	(0.27)	(0.26)	(0.39)	(0.19)	(0.18)
N	60	60	60	60	60	60

Table 1.8: Efficiency of insider payoffs. Standard deviations are in parentheses.

	Zero Monitoring		Partial Monitoring		Full Monitoring	
	Initial	Final	Initial	Final	Initial	Final
Periods 1-7	0.32	0.32	0.46	0.32	0.59	0.42
	(0.30)	(0.30)	(0.50)	(0.65)	(0.48)	(0.35)
Periods 8-15	0.58	0.58	0.44	0.27	0.54	0.24
	(0.48)	(0.48)	(0.39)	(0.52)	(0.49)	(0.40)
All Periods	0.46	0.46	0.45	0.29	0.56	0.32
	(0.43)	(0.43)	(0.44)	(0.58)	(0.49)	(0.39)
N	60	60	60	60	60	60

Table 1.9: Efficiency of outsider payoffs. Standard deviations are in parentheses.

Over all periods, average final efficiency for insiders was highest in the Full Monitoring treatment ($p = 0.06$ for the comparison to the Zero Monitoring treatment and $p = 0.00$ for the comparison to the Partial Monitoring treatment).[14] Moreover, these significant differences are consistent in the first and second halves of the sessions and when I examine differences in initial and final efficiency. Recall that complete monitoring of outsiders allowed the insiders to limit poaching to lower and more stable levels, and as a result the insiders were able to better coordinate their harvests. In

[14]For both insider and outsider efficiency I use Wilcoxon nonparametric tests to compare group efficiency across and within treatments. The results are consistent with those obtained from OLS regressions with corrected standard errors.

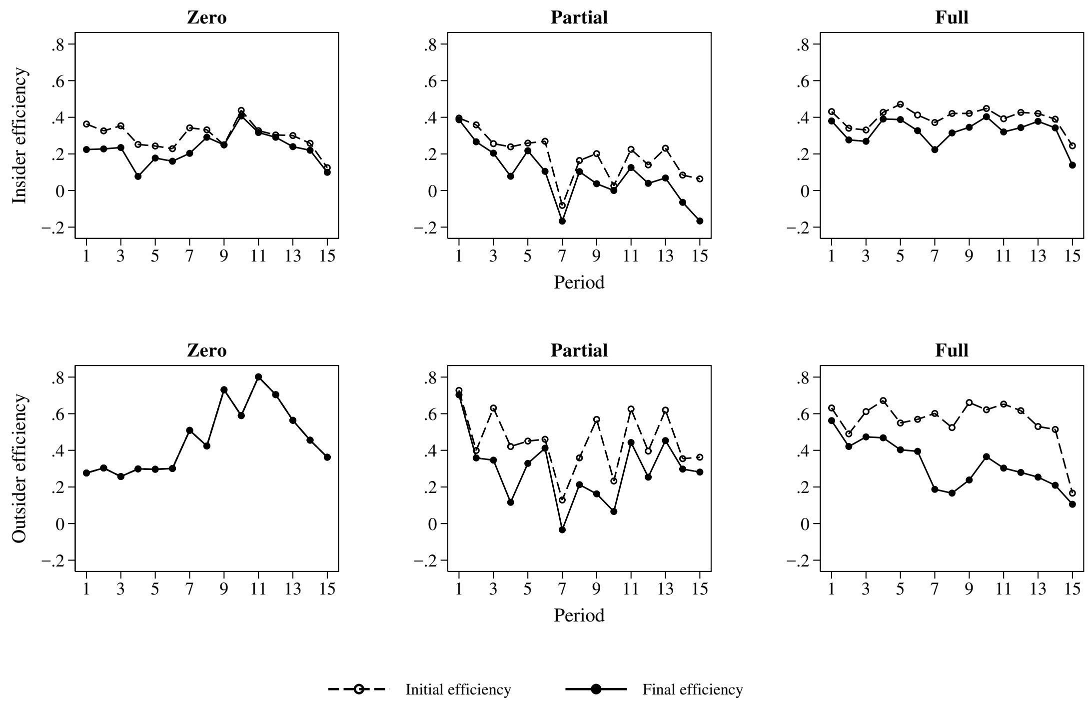

Figure 1.4: Average initial and final efficiency of insiders (A) and outsiders (B).

turn, insiders were able to achieve higher and more consistent payoffs in the Full Monitoring treatment than in the Zero Monitoring and Partial Monitoring treatments.

However, recall that increasing the ability of the insiders to monitor and sanction the outsiders from Zero Monitoring to Partial Monitoring actually led to more poaching. This non-monotonic effect of increased monitoring is mirrored in a non-monotonic effect of increased monitoring on insider welfare. In fact, over all periods, final average efficiency levels in the Partial Monitoring treatment are significantly lower than in the Zero Monitoring treatment ($p = 0.01$). This difference emerges over time. The difference in average final efficiency between the Zero Monitoring and Partial Monitoring treatments is not significant for Periods 1-7 ($p = 0.74$), but efficiency under the Zero Monitoring treatment is significantly lower in Periods 8-15 ($p = 0.00$). While final efficiency in the Zero Monitoring treatment increased from the first to the second half of the sessions, this increase is not significant ($p = 0.41$). Moreover, the time series in the top-left panel of Figure 4 suggests that I should not conclude that final efficiency in Zero Monitoring rose consistently over time. In contrast, it is clear that final efficiency in the Partial Monitoring treatment eventually fell to very low levels.

In fact, average final efficiency for the insiders in the Partial Monitoring treatment was very nearly zero in Periods 8-15 (0.02 from Table 1.8), and the time series in the top-middle panel of Figure 4 shows that average final efficiency across groups in this treatment fell below zero in three periods. This is significant because a value of zero means that insider payoffs were no different than in the Nash equilibrium. In other words, our results suggest that attempts by insiders to coordinate their harvests and limit poaching under limited monitoring and sanctioning of outsiders led to payoffs than were not much different from payoffs under a theoretical model in which they could not coordinate their harvests or sanction poaching.

1.4.3.2 Outsider efficiency

Finally consider outsider efficiency. According to the payoffs contained in Table 1.2, outsider payoffs are highest in the C/ND outcome in which insiders coordinate their harvests but are unable to deter poaching. In this case the insiders create surplus by restraining their harvests which is then taken by the outsiders. Our efficiency measure for the outsiders is the ratio of the difference between their observed payoffs and their symmetric payoffs in the NC/ND outcome to the difference between their symmetric payoffs in the C/ND and NC/ND outcomes; that is,

$$E_{ok} = \sum_{h=1}^{n_o} \left(\frac{\pi_{ohk}^{Observed} - \pi_o^{NC/ND}}{\pi_o^{C/ND} - \pi_o^{NC/ND}} \right), \; k = (initial,\; final). \tag{1.10}$$

In (1.10), $\pi_{ohk}^{Observed}$ is the initial or final payoff of an outsider h in a period, $\pi_o^{NC/ND}$ is the symmetric individual payoff for outsiders in the Nash equilibrium, and $\pi_o^{C/ND}$ is the symmetric outsider payoff in the C/ND outcome. I use the benchmark NC/ND

as the floor for outsider efficiency because only about 4% of all final and initial payoffs for outsiders across treatments match the theoretical worst-case payoff for outsiders, the C/D outcome. Finally, since insider and outsider efficiency measures are calculated differently, it is not appropriate to compare efficiency values across the subject types.[15]

The time series for the average of (1.10) over outsider groups is displayed in the bottom panels of Figure 1.4, and the average over groups and time are in Table 1.9. Perhaps not surprisingly, the outsiders were better off in the Zero Monitoring treatment than the other treatments. Over all periods, average final efficiency in Zero Monitoring is significantly higher than in Partial Monitoring ($p = 0.06$) and Full Monitoring ($p = 0.03$). However, this ranking is entirely dependent on the lack of sanctions in the Zero Monitoring treatment. Average initial efficiency in the Full Monitoring treatment is significantly higher than in the Zero Monitoring treatment ($p = 0.02$) and Partial Monitoring treatment ($p = 0.06$), while the difference in average initial efficiency between the Partial Monitoring and Zero Monitoring treatments is not significant ($p = 0.75$). The higher initial payoffs for outsiders in the Full Monitoring treatment are due to reduced harvests by both insiders and outsiders in the treatment. However, the sanctions used by the outsiders to partially deter the outsiders in this treatment led to lower payoffs for the outsiders than in the Zero Monitoring treatment.

[15] I also do not think one should interpret (1.10) as an absolute account of how well the outsiders performed. The baseline outcomes C/ND and NC/ND are largely beyond the outsiders' control, because they depend on the ability of the insiders to coordinate their harvests and to deter the outsiders. In addition, the outsiders do not have explicit mechanisms to coordinate their harvests. I can, however, use differences in average E_{ok} to evaluate treatment and time differences in the welfare of the outsiders.

1.5 Conclusion

I have presented the results of a common pool resource experiment that includes a group of outsiders who can poach the resource. Perhaps the most important lesson from our study is that common pool resource users may often have a very difficult time deterring outside encroachment. The main reason for this difficulty in our experiments is that the insiders were unable or unwilling to spend enough on sanctioning poaching. This might be because they did not understand what is required to make the expected sanction for poaching exceed the gain, but it seems more likely that they could not coordinate their sanctioning effort effectively enough to erect a sufficient deterrent.

Our results also suggest that the consequences of insufficient deterrence are likely to be most severe in realistic settings in which outside threats are observed imperfectly. In our experiments, outsider harvests were actually higher when they were observed imperfectly than when they could not be observed or sanctioned. Consequently, the insiders were much worse off when they could monitor and sanction outsiders imperfectly than when they had no ability to defend the resource. In fact, they were not better off under imperfect monitoring of poaching than in the theoretical Nash equilibrium in which they could not deter outsiders at all or coordinate their own harvests.

On the other hand, our results suggest that a high level of observability of outside threats can help a group deter poaching to a limited degree and better coordinate their own harvests. However, the non-monotonicity between observability and deterrence I observe is troubling, because it suggests that simply increasing the ability to monitor outside threats may not lead to better outcomes, and it leaves us unsure about what level of observability is required to generate better outcomes. Clearly, more research is necessary to explore the relationship between observability of outside threats and the ability of a group to coordinate its actions.

Moreover, there are mechanisms that might help insiders coordinate their defense of the resource more effectively that I have not explored in this chapter, but that I think would be valuable extensions of our work. One simple extension would be to give the insiders better information on sanctioning choices within their group. As I noted earlier, I adopted a standard design for allowing monitoring and sanctioning of individuals, which does not reveal information about who sanctions whom. Better information about these choices may help insiders coordinate their sanctioning of outsiders. There are also commitment devices that might help insiders defend their resource better. For example, designing and voting on specific responsibilities for providing deterrence might help coordinate sanctioning efforts more effectively than the decentralized provision of defense that I allow in our experiments.

Many potentially informative extensions of our work involve exploring the role of property rights in the experiments. Recall that our experiments did not assign the exclusive right to the resource to the insiders, and outsider harvests were not framed as taking or stealing the insiders' property. However, using an explicit property rights frame in future experiments may enhance the insiders' ability to defend the resource. Framing the decision environment as insiders defending their property may help them coordinate deterrence more effectively, and the outsiders may show more restraint if their actions are framed as taking someone else's property. A related extension would be to explore how having insiders earn their position affects their choices. In our experiments, the assignment of the asymmetric rights to communicate and sanction others was determined randomly. However, there is a significant literature on the effects of whether endowments or experimental roles are assigned by the experimenter or earned by the subjects. See, for example, Hoffman et al. (1994), Cherry et al. (2005), and Oxoby and Spraggon (2008). This literature is relevant to our experiments, because the insiders might have made more of an effort to deter the outsiders if they had earned their privileged rights as insiders. Another useful extension concerning property rights would be to give the insiders the ability to invest in the productivity of the resource. Such investments do occur in reality, and they may have interesting effects on the defense of common pool resources. It is possible that the ability to invest in resource productivity would motivate insiders to also invest more in deterring poaching to protect their investments. On the other hand, the threat of poaching may weaken the insiders' motivation to invest in the first place.

Another set of extensions could investigate differences in the "technology" of deterrence. For example, in our study, observability of outsiders was exogenous and free: insiders chose sanctions, given their exogenous ability to observe the outsiders. In other settings, individuals may need to coordinate monitoring rather than sanctions. For example, neighborhood watch programs to deter criminal behavior in neighborhoods seem to rely on coordinated monitoring, while sanctioning is left to local authorities. In fact, vigilante punishment would likely be illegal in this case. It may be worthwhile to explore individuals'ability to coordinate monitoring to deter outsiders, given that sanctions are provided by the state. Another possible extension of our work is to examine the role of the state in providing observability or sanctions. In our study there is not a role for the state, but in some situations individuals and the state may combine efforts to provide deterrence. A central question in this context is whether the state's provision of enforcement activities complements or is a substitute for individual efforts.

CHAPTER 2

DEFENDING PUBLIC GOODS AND COMMON-POOL RESOURCES

2.1 Introduction

Economic theory predicts that self-interested agents will inefficiently provide public goods (PG) and conserve common-pool resources (CPR) due to the incentive to freeride on the cooperation of others. Nevertheless, examples of people creating surplus through cooperation in many real-life social dilemmas abound. Fishing co-operatives like Territorial User Rights Fisheries (TURFs) around the world protect stocks of fish from overexploitation (Deacon, 2012; Wilen et al., 2012) . Individuals readily donate money to charity in various ways, from online donations, large non-profit organizations, and cash in the church tillbox. In addition, there is now a well-developed literature in experimental economics that finds high if not optimal levels of cooperation among subjects in laboratory social dilemmas with peer monitoring and punishment (Chaudhuri, 2011)

However, agents who successfully diminish the gains to freeriding may face a new problem. If rights to the surplus are ill-defined or unprotected, in-group cooperation can increase the incentives for outsiders to encroach. Fishermen who restrict harvests can invite poaching (FAO et al., 2012). Donations to charities turn them into targets for cyberthieves.[1] Despite the extensive literature on cooperation in decentralized

[1]Charities and nonprofits are often less protected against cyberattacks than the public and private sectors. A recent report on cybersecurity infrastructure in the United States found that nonprofits invested significantly less in preventative measures than government and private industry (Rowe et al., 2013).

groups, little is known about how these groups protect the gains from cooperation. This paper aims to fill this gap with a laboratory experiment that examines the coordination of in-group contributions to a PG and management of a CPR threatened by outsider theft.

Exactly how external threats affect group behavior is unclear. The risk of theft could fuel self-interest and prompt a collapse in collective action. Alternatively, enemies at the gate could rally insiders. Collective action in this case could increase as group identity becomes more pronounced, such as when people make charitable donations following a disaster. Before a threat is realized it is difficult to know which will happen. Furthermore, it is possible that the outcome hinges on whether people's actions confer positive externalities, in the case of a PG, or negative externalities, in the case of a CPR.[2]

I report the results of an experiment intended to separate the effects of an external threat on decentralized PG provision versus CPR management. Our design extends the design of payoff equivalent and strategically symmetric games of Apesteguia and Maier-Rigaud (2006) and Kingsley and Liu (2014) to include an external threat. Our control follows the two studies by allowing a group of four insiders to provide a PG or manage a CPR without risk of theft. I then introduce a group of two independent outsiders who observe the actions of the insiders and choose whether to take some or all of their surplus. Insiders could attempt to deter the outsiders using costly sanctions. Our main goal in this study is to determine how the introduction of theft affects the surpluses generated by cooperative allocations to group accounts, and whether these effects are different in the PG and CPR settings.

[2]The supply of deterrence can also vary depending on the externalities generated. In the case of cybersecurity, actions taken towards deterring cybertheft can confer positive externalities, for example when one secures a server and thereby protects other computers connected to it, or negative externalities, such as when one erects a firewall which then divert attacks to networks with less protection (Rosenzweig, 2011).

Our controls successfully replicate Kingsley and Liu (2014) that there is not a significant difference in average levels of cooperation in the PG and CPR settings in the absence of theft. However, the presence of theft led the CPR users to cooperate more in terms of group allocations, while theft had no effect on average cooperation by PG providers. Moreover, perhaps because the CPR users generated greater surpluses, losses to theft were significantly higher in the CPR setting than in the PG setting. Despite this, CPR users produced greater average surpluses net of theft than the PG providers. I also found interesting differences between the CPR and PG settings in terms of the consistency of individual allocations to group accounts. In particular, variation in individual contributions to group accounts was higher in the PG setting than in the CPR setting, both in the absence and presence of theft. Moreover, the introduction of theft increased the variation of individual contributions in both settings. In sum, our results suggest that the finding that there is no difference in cooperation in payoff equivalent and strategically symmetric PG and CPR games must be revised when the surpluses generated in these games are subject to theft.

The rest of the paper proceeds as follows. In section 2 I review the related literature. In section 3 I present our experiment design and procedures. I summarize the results of our experiment in section 4. Finally, section 5 concludes.

2.2 Related literature

There is a well-developed literature that uses experiments to study behavior in social dilemmas. Following the lead of (Ostrom et al., 1992), who studied decentralized CPR management, and (Gächter and Fehr, 2000), who studied decentralized public good provision, this literature has primarily focused on sustaining cooperation in CPRs and public good settings using communication and both formal and informal sanctions (see (Chaudhuri, 2011) for a review).

With time the literature treated CPRs and public goods as two sides of the same prisoner's dilemma coin. In reality they differ in two important ways. First, consumption of a CPR is rival while public consumption is not (Apesteguia and Maier-Rigaud, 2006). Second, the externality of an agent's actions are either negative (CPR) or positive (PG). (Andreoni, 1995) was the first to show that framing matters. Levels of cooperation are higher in a game with positive externalities than in a strategically equivalent and symmetric game with negative externalities. Several studies on the so-called cooperation divergence have since offered mixed findings. Here I review that literature.

Our contribution to this literature is to add outsiders who can take the surplus created in PG and CPR settings. To this end I also review the small CPR literature in which a group of outsiders can poach the resource.

2.2.1 Cooperation under negative and positive externalities

The cooperation divergence between PG and CPR settings has been attributed to how cooperation in a social dilemma is framed. Cooperating in a public good game means giving some portion of an endowment to a shared account. Cooperating in CPR game means using an endowment to take from the shared account. Framing cooperation as giving versus taking should not have any impact on behavior so long as incentives are unchanged. But when agents in strategic settings not only choose actions but form beliefs too, framing can influence behavior in games with identical explicit incentives (Tversky and Kahneman, 1981). This is because strategic environments may house relevant information (sometimes known as cues or reference points) separate from the actions of others that an agent can draw upon when deciding what action to take (Mehta et al., 1994; Schelling, 1980). For example, Croson (2007) and Fischbacher and Gachter (2010) find that voluntary contribution in simple public goods games is positively correlated with beliefs about cooperation, even though beliefs about actions do not affect explicit incentives.

Research on framing effects on behavior in social dilemmas has focused on two types of framing. Label framing involves using different vocabulary to describe a game but leaving the incentives and reference points unchanged. For example, Ellingsen et al. (2012) replicate Ward et al. (1997) and Liberman et al. (2004) and find subjects cooperate more in a prisoner's dilemma if it is framed as a "Community Game" rather than a "Stock Market Game". Valence framing, on the other hand, involves framing the action taken by a subject as creating a positive externality ("giving") or a negative externality ("taking"). It is this type of framing to which I turn our attention.

Andreoni (1995) was the first to explain the cooperation divergence as the result of valence framing. The author pointed out that lower levels of cooperation in CPR games compared to public good games were a general feature in social dilemma experiments. To test if cooperation hinged on how it was framed, he ran a study in which subjects used their endowment to give (take) to (from) a shared account under incentives that promoted non-cooperation as a dominant strategy. Subjects were found to cooperate more when giving rather than taking. Framing cooperation was argued to capture the idea that people are more stimulated to cooperate when there is a benefit to be had rather than a cost to be avoided. A person whose actions confers a group benefit is rewarded with a nebulous internal benefit, "warm glow". But if her action instead imposes a cost, she forgoes warm glow and instead gets a "cold prickle".

In these terms, contributions to a public good are actions that generate positive externalities and warm glow. Extraction from a common pool, on the other hand, are actions that generate negative externalities and cold prickle. Andreoni found that even when incentives are identical in both settings, such that subjects who are purely self-interested will not cooperate, the possibility of receiving warm glow, as opposed to the possibility of avoiding cold prickle, drives cooperation among subjects. Cooperation thrives when agents can receive warm glow but not when they have to avoid cold prickle. Andreoni (1995) argues this may be the reason why charities induce donations by emphasizing the warm glow of cooperating (donating) and not the cold prickle of freeriding (not donating).

Many ensuing studies have corroborated giving as the cooperation stimulant. Park (2000) replicates Andreoni (1995) while controlling for subjects' value orientation (a social psychology measure of an individual's preferences for her welfare and the welfare of others), finding that subjects who are more individualistic cooperate significantly less in the negative frame treatment. Cookson (2000) finds an increase in voluntary contributions when the payoff function is framed in terms of a gift to be distributed among members of group as opposed to the standard and equivalent direct benefit from contributions. Cox (2015) replicates Andreoni (1995) with an enhanced 2x2 experiment design that extends the traditional Giving and Taking treatments with two additional treatments, Gains and Losses. While Andreoni (1995) only looked at cooperation when agents contributed to generate gains or take to generate losses, Cox (2015) controls for gain-loss framing, a possible confounder, by looking at contributions to generate losses and taking to generate gains. He finds significantly less cooperation in both Taking frames. Yet gain-loss framing had little effect, suggesting that the cooperation divergence is due entirely to externality framing.

Some studies have found the cooperation diverge not in actions but in beliefs. Dufwenberg et al. (2011) point out that framing influences behavior indirectly: frames influence subject beliefs about an environment and beliefs influence her motivations. The authors use a simple, one-shot public goods game where motivations depend on beliefs to show that framing indirectly affects contributions by directly affecting first-order beliefs (beliefs about others' actions) and second-order beliefs (beliefs about others' beliefs). They find that while levels of cooperation are relatively similar across games, beliefs about the cooperative nature of other players are significantly higher in the positive externality setting.

However, other studies that altered the implicit assumptions of Andreoni (1995) have failed to find any differences in behavior in public good and common-pool resource games.[3] Khadjavi and Lange (2015) replicates Andreoni (1995) and found that the cooperation divergence is sensitive to the parameters. A significant increase in cooperation in the positive externality treatment is driven by the starting provision of the public good and the size of the choice space. Subjects playing a linear public goods game contribute less when the baseline provision of the public good is positive and when the choice space includes more "take" options. Stoddard (2013) likewise find no cooperation divergence in repeated appropriation and provision games until surplus uncertainty is introduced, at which point cooperation in the provision game suffers.

Casari and Plott (2003) find that the cooperative divergence may emerge only when agents are symmetric. The authors argue that agents in public goods games appear altruistic while agents in common-pool resource games appear spiteful, when in reality each setting will contain a mixture of types. The authors consider a social dilemma where agents have heterogeneous and linear other-regarding preferences, relaxing the assumption of identical agent behavior by allowing agents to be selfish, altruistic or spiteful. Results from a public good and common-pool resource experiment built around their model suggest that the presence of spiteful individuals, in combination with the location of the Nash equilibrium in the choice space, is sufficient to explain the cooperation divergence. Where the Nash equilibrium lies determines

[3]Not all studies on the cooperation divergence replicate the original study by (Andreoni, 1995). For example, Cubitt et al. (2011) found no significant difference in cooperation in one-shot public good games with framing. The authors note that one-shot games and not repeated games are better suited to identify the *existence* of a framing effect, rather than the *persistence* of a framing effect over time.

the leverage of spiteful individuals. When spiteful individuals have more leverage in either environment, they can drive down average cooperation. Finally, Cox et al. (2013) observe a cooperation divergence only under power asymmetry. They find similar levels of efficiency in generalized symmetric and payoff-equivalent one-shot appropriation (e.g. ultimatum game, common-pool resource game) and provision (e.g. public good game, trust game) games. When they allow some subjects to be more powerful than others, they observe bigger efficiency losses in the appropriation games.

Still other studies have reconsidered the strategic set-up of Andreoni (1995). Under particular scrutiny is the idea that a public good game and a CPR game should be treated as strategically and payoff equivalent. Two games are considered to be strategically equivalent if they generate the same best-response functions and in turn the same level of equilibrium effort (Chowdhury and Sheremeta, 2015). (They are payoff equivalent if equilibrium actions generate the same payoffs). Assuming strategic equivalence would imply identical best-responses in the CPR and public good game. Yet this is clearly untrue. Equilibrium appropriation will be high (the full endowment if payoffs are linear) in the CPR game. Equilibrium provision in the public game, however, will be low.

In line with this observation, Apesteguia and Maier-Rigaud (2006) earlier pointed out that many studies mistakenly treat a public good game the same as a common-pool resource game, going so far as to label common-pool resources as a framed public good. In reality the degree at which the two types of goods differ depends on their respective degree of rivalry. Rivalry can be manipulated in the lab by varying the distributional parameter of the group payoff function. For example, a common-pool resource unchanged is rival, but if an institution is imposed that requires total individual catch to be distributed equally in a population, it is now a non-rival public good.

Apesteguia and Maier-Rigaud (2006)run a public goods and common-pool resource experiment by holding constant payoffs to the public good and varying the distributional factor and thus the rivalry of the common-pool resource, thus varying the degree to which one game is a mirror of the other. The authors find that subjects are sensitive to changes in rivalry, leading to a different investment decisions but identical payoffs. In this way the authors argue that behavior is qualitatively similar between games. However, the experiment design did not explicitly show subjects the externality implications of their investment decisions on the payoffs of others. Kingsley and Liu (2014) implement the exact same experiment design as Apesteguia and Maier-Rigaud (2006), but make explicit this information by showing group payoff tables so as to match the design of (Andreoni, 1995). The authors find that giving subjects this information does not change the results of Apesteguia and Maier-Rigaud (2006).

2.2.2 CPRs with poaching

A prevailing assumption in the experimental literature on social dilemmas is that the gains from cooperation are safe from poaching. However, many fishing collectives often have to manage in-group overexploitation and at the same time deter out-group poaching, and often they struggle to do both (Muchapondwa et al., 2014; Gelcich et al., 2010). Despite the intense focus on decentralized social dilemmas, there is surprisingly little research done on how outside threats affect group coordination. (Schmitt et al., 2000) allow poaching of a common pool resource. The authors find that the threat of poaching decreased insider coordination. However, the authors do not allow insiders to deter the outsiders, and therefore they do not address the trade-off insiders make between coordinating their harvest decisions and defending the resource.

To the best of our knowledge, De Geest et al. (2017) are the first to run a CPR experiment with poaching and deterrence. The authors include a group of outsiders who can poach a resource managed by a group of insiders, who in turn can deter the outsiders with sanctions. Monitoring of outsiders is varied across treatments, while insiders are able to perfectly monitor and sanction each other. The authors find a non-monotonic effect of monitoring on insider welfare. Insiders maintain stable levels of harvest across treatments, but expenditures on sanctioning outsiders and insiders led to lower insider efficiency in a limited monitoring treatment compared to a no-monitoring treatment. While insider efficiency improved in a full monitoring treatment, insiders were unable to coordinate their efforts sufficiently to completely deter poaching.

It is important to note that these studies involve outsiders poaching the resource itself, and thus they may take the input into team production. Our experiments have outsiders taking the surplus created in PG and CPR settings. I designed our experiments with this feature to make the PG and CPR settings strategically symmetric.

2.3 Design & procedures

2.3.1 Theoretical model

In this section I outline a theoretical model of insiders who play a social dilemma with negative or positive externalities. Insiders choose whether to engage in collective action to produce a surplus. Outsiders observe the insiders and choose whether to steal their surplus. Since I am interested in exploring differences in insider behavior when actions generate either positive or negative externalities, theft refers to stealing the surplus.

Insiders choose between investing in a private good with a fixed return or either contributing to a public good (+) or extracting from a CPR (−). I use plus and minus signs to signify that the actions of an insider impose positive and negative externalities on the other insiders, respectively. Following Apesteguia and Maier-Rigaud (2006), let the payoffs to the public good π^{+}_{hi} and the CPR π^{-}_{hi} for insider h in the group of insiders be:

$$\pi^{+}_{hi} = c(e_i - g_{hi}) + \frac{1}{n_i}V(G_i) \tag{2.1}$$

$$\pi^{-}_{hi} = c(e_i - g_{hi}) + \frac{g_{hi}}{\sum_{h=1}^{n_i} g_{hi}}V(G_i). \tag{2.2}$$

In (1) and (2), e_i is each insider's common endowment, g_{hi} is the agent's investment choice (harvest of the CPR or contribution to the public good), c is the fixed return to the private good, $V(G_i)$ is the cooperation surplus or the value of collective action defined by $a\sum_{h=1}^{n_i} g_{hi} - b(\sum_{h=1}^{n_i} g_{hi})^2$, and n_i is the number of insiders. Incentives create a tension between group interest and self-interest when $a > c > b$ and $0 < b < 1$. Note that if I impose symmetry, say in the form of a equal-split institution such that $\sum_{i=1}^{n_i} g_{hi} = ng_{hi}$ then $\pi^{+}_{i} \equiv \pi^{-}_{i}$ in the sense that the shared output is simply divided equally among insiders, and thus the once-rival CPR now becomes a non-rival public good. This captures the idea that the main separation between a shared input and a shared output is the degree of rivalry captured by $g_{hi}/\sum_{i=1}^{n_i} g_{hi}$.

I first solve this model for non-cooperative and non-cooperative aggregate values of G_i assuming no outsider theft. Solving for the aggregate value of G_i under Nash strategies yields

$$G_i^{N+,D} = \frac{a - cn_i}{2b} \tag{2.3}$$

$$G_i^{N-,D} = \frac{n_i(a - c)}{b(n_i + 1)}. \tag{2.4}$$

The aggregate social optimum for both games is

$$G_i^{S+,D} = G_i^{S-,D} = G_i^{S,D} = \frac{a - c}{2b}. \tag{2.5}$$

The public good and CPR scenarios are *strategically symmetric* in that they share the same social optimum while their respective Nash equilibria are equidistant in absolute value from the social optimum. Finally, because $G_i^{N-} - G_i^{S-} = \left|G_i^{N+} - G_i^{S+}\right|$, the two games are payoff equivalent.[4]

Next I consider outsider payoffs. Outsiders choose between a private good with fixed return and a second private good with variable return that depends on the insider surplus. For this reason an outsider's payoff function will be the same in the public good and CPR environment. Let the payoff to outsider h be

$$\pi_{ho} = c(e_o - x_{ho}) + f(x_{ho}, V(G_i)) \tag{2.6}$$

where $f(x_{ho}, V(G_i)) = x_{ho} w V(G_i)$ governs the returns to effort x_{ho} exerted towards theft at cost $0 < w < 1$. All the outsiders have the same endowment e_o. For simplicity I assume that a given outsider can only poach the cooperation surplus from a single group of insiders. Since an outsider's payoff is linear in x_{ho}, he will either choose to expend all his endowment on taking the insiders' surplus or on investing in the fixed-return private good. Maximizing payoff with respect to effort, the first-order condition for an outsider is

$$-c + w(aG_i - bG_i^2) \begin{cases} \geq 0, \text{ if } > 0, \text{ then} & x_{ho} = e_o \\ \leq 0, \text{ if } < 0, \text{ then} & x_{ho} = 0. \end{cases} \tag{2.7}$$

If this holds as an equality it has a solution in G_i with two roots:

$$G_i^0 = \frac{a}{2b} - \frac{\sqrt{w\left(a^2 w - 4bc\right)}}{2bw}, \; G_i^1 = 2b + \frac{\sqrt{w\left(a^2 w - 4bc\right)}}{2bw}, \tag{2.8}$$

[4]The games are not *strategically equivalent* because they do not generate the same best-response functions and equilibrium levels of effort (Chowdhury and Sheremeta, 2015).

The function $V(G_i)$ and these roots are shown below in Figure 1. The parameters I use in our experiments guarantee that $G_i^0 > 0$. $V(G_i)$, reaches a global maximum at $a/2b$. Since this value is greater than the socially optimal $G^{S+} = G^{S-}$, it is reasonable to assume that insider will not *collectively* choose a level of G_i higher than the level that maximizes their surplus. Therefore I can write the outsider's decision rule as

$$x_{ho} = \begin{cases} 0, & \text{if } G_i \leq G_i^0 \\ e_o, & \text{if } G_i \geq G_i^0, \end{cases} \tag{2.9}$$

with payoffs

$$\pi_{ho} = \begin{cases} ce_o, & \text{if } G_i \leq G_i^0 \\ e_o w(aG_i - bG_i^2), & \text{if } G_i \geq G_i^0. \end{cases} \tag{2.10}$$

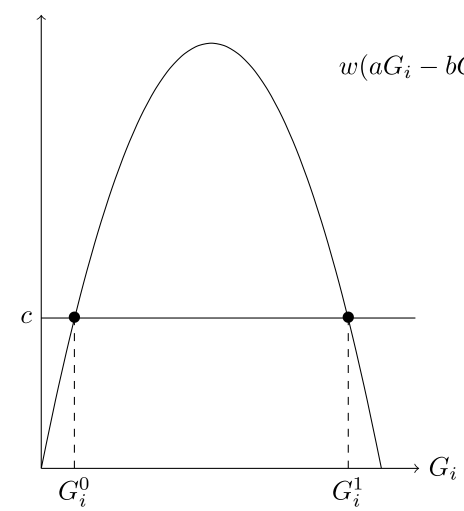

Figure 2.1: Outsider payoffs from theft and investing

2.3.1.1 Deterrence

Suppose now that insiders have a deterrence technology θ that converts a sanction by an insider s_{hi} into a monetary punishment θs_{hi} on an outsider. Since insiders can only punish outsiders by reducing their payoffs after they have chosen how much of the insiders' surplus to take, a particular punishments becomes a fixed component of an outsider's payoff. Therefore, the threat of punishment will either fully deter individual poaching or not at all. Moreover, since individual insiders have little incentive to unilaterally punish others, to deter poaching the insiders must collectively commit to a punishment threat.

Given $G_i \geq G_i^0$, and assuming insiders can monitor n_m of n_o outsiders, insiders who generate a surplus of $V(G_i)$ can deter outsiders by collectively imposing a punishment

$$\theta \sum_h s_{hi} \geq \left(\frac{n_o}{n_m}\right) e_o w V(G_i). \tag{2.11}$$

This level of sanctions eliminates all the gains from poaching times the reciprocal of the probability an outsider is monitored. If the insiders can commit to this punishment scheme, then aggregate levels of insider harvests or public good contributions G_i are simply (3) and (4) and the outsiders invest their entire endowment into the fixed-return private good. If the insiders fail to completely deter the outsiders, then they are not deterred at all. Making the strong assumption that the insiders' cost of coordinating their punishment strategy is zero, note that insiders never actually incur sanctioning costs. If the outsiders are fully deterred there is no reason to punish them, and if they are not deterred then the optimal sanction is zero.

2.3.1.2 Non-deterrence

When insiders do not commit to deterrence, outsider theft $x_{ho} = e_o$ enters the insider's objective functions. Substituting theft into aggregate insider payoffs

$$\sum_i \pi_h i = n_i c e_i - cG_i + V(G_i) - e_o w V(G_i), \tag{2.12}$$

and maximizing with respect to G_i yields the socially optimal aggregate levels of harvests or contributions when the insiders cannot deter the outsiders,

$$G_i^{S-,ND} = G_i^{S+,ND} = \frac{a(1 - e_o w) - c}{2b(1 - e_o w)}. \tag{2.13}$$

These are positive values only if $1 - e_o w > 0$, which I assume throughout. Given $1 - e_o w > 0$, aggregate cooperative harvests/contributions are declining in poaching e_o. This is intuitive because poaching reduces the return to cooperation.

To solve for the non-deterrence Nash outcomes, I assume that the losses from outsider theft are equally split between the insiders. I can then write the individual insider payoff functions as

$$\pi_{hi}^{+} = c(e_i - g_{hi}) + \frac{1}{n_i}V(G_i) - \frac{1}{n_i}e_o w V(G_i) \tag{2.14}$$

$$\pi_{hi}^{-} = c(e_i - g_{hi}) + \frac{g_{hi}}{G_i}V(G_i) - \frac{1}{n_i}e_o w V(G_i), \tag{2.15}$$

and find the aggregate Nash levels of G_i

$$G_i^{N+,ND} = \frac{a(1 - e_o w) - c n_i}{2b(1 - e_o w)} \tag{2.16}$$

$$G_i^{N-,ND} = \frac{n_i\left(a(1 - \frac{1}{n_i}e_o w) - c\right)}{b(n_i + 1 - 2e_o w)}. \tag{2.17}$$

2.3.1.3 Comparing benchmarks

Giving values to the parameters of our model allows us to derive and compare benchmarks outcomes under different assumptions about insiders' abilities to coordinate their harvest/contribution decisions and their ability to deter the outsiders. Most parameter values come fromKingsley and Liu (2014), who assume $a = 6, b = 0.025, c = 1, e_{hi} = e_{ho} = 50$ and $n_i = 4$. In addition, to satisfy $1 - e_o w > 0$, I set $w = 0.01$.

Table 2.1 summarizes insider payoffs from G_i^0 and the four main benchmarks for the public good and CPR environments. The table shows that the gains from insider cooperation are tied to outsider deterrence. When outsiders are not deterred, insider gains from cooperation, while still greater than non-cooperation, are substantially reduced.

Benchmark	G_i	g_{hi}	π_{hi}
G_i^0	18	4.5	70
$G_i^{S-,D}$	100	25	112.5
$G_i^{N-,D}$	160	40	90
$G_i^{S-,ND}$	80	20	70
$G_i^{N-,ND}$	170	42.5	45

(a) CPR

Benchmark	G_i	g_{hi}	π_{hi}
G_i^0	18	4.5	70
$G_i^{S+,D}$	100	25	112.5
$G_i^{N+,D}$	40	10	90
$G_i^{S+,ND}$	80	20	70
$G_i^{N+,ND}$	0	0	50

(b) Public Good

Table 2.1: Insider benchmarks and payoffs

Finally, Table 2.2 summarizes outsider payoffs from insider benchmarks. Payoffs to outsiders deterred are simply the entire endowment invested into the fixed-return private good. However, outsider payoffs are maximized when insiders cooperate but fail to deter the outsiders. In this case the outsiders poach the entire surplus net their disutility of effort.

Benchmark	π_{h0}
G_i^0	50
$G_i^{S-,D}$	50
$G_i^{N-,D}$	50
$G_i^{S-,ND}$	160
$G_i^{N-,ND}$	110

(a) CPR

Benchmark	π_{h0}
G_i^0	50
n $G_i^{S+,D}$	50
$G_i^{N+,D}$	50
$G_i^{S+,ND}$	160
$G_i^{N+,ND}$	50

(b) Public Good

Table 2.2: Outsider payoffs from insider benchmarks

2.3.2 Experimental procedures

Kingsley and Liu (2014) adapt the design of Apesteguia and Maier-Rigaud (2006) to explicitly inform subjects how their decisions affect group earnings. While only Kingsley and Liu (2014) provide a metric to directly compare cooperation under positive versus negative externalities, both studies find no evidence for a cooperation divergence. Since I am interested in how the threat of out-group theft affects in-group coordination, it is useful to begin from a baseline where there are no differences in cooperation across environments and then introduce the external threat. Differences in group cooperation across environments in response to the threat can then plausibly be attributed to the threat.

I employ a 2x2 design, {CPR, Public Good}x{No Theft, Theft.} The no-theft treatments replicate Kingsley and Liu (2014) and are similar to social dilemma games with nonlinear payoffs (e.g. Cason and Gangadharan (2015)). Since I expected to see no divergence in cooperation, these treatments served as our controls.

Subjects were divided into independent groups of four and remained in their groups for the duration of the experiment (partners design). Groups played fifteen rounds of either a CPR or public goods game. To avoid any uncontrolled framing effects, both games were referred to as a "Decision making experiment".

At the start of a round, subjects received an identical endowment e and decided how much of their endowment to invest in a private good with fixed returns or a public account, Account 1, whose return varies with the investment decisions of all the group members. To make sure subjects understood how their investment decisions would impact not only their payoffs but the payoffs of their group members, individual and group payoff tables were provided in the instructions. At the conclusion of the investment round, subjects were then informed of their final payoffs as determined by either (1) or (2) depending on whether they were in the public good or CPR treatment. Figure 1 summarizes the timeline of stages in the no-theft (control) treatments.

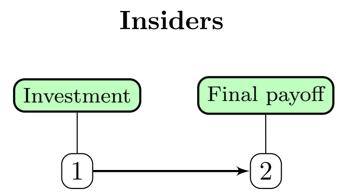

Figure 2.2: Timeline of stages in the no-theft treatments

Figure 2 summarizes the timeline of stages in the theft treatments. These treatments varied from the controls by introducing two outsiders who could poach the surplus created by the four insiders.

Subjects were divided into groups of six. Within each group, subjects were then divided into an in-group of four or an out-group of two. Insiders and outsiders received the same endowment at the start of each round. Once the insiders finished making their investment decisions, outsiders were informed of the value of the in-group's aggregate investment decision (i.e., total insider payoffs from their group account). Outsiders could then decide how much of their endowment to invest in a fixed-return private good or in a "take" from the in-group's surplus with a fixed disutility of effort w. I constrained each outsider to take at most 25% of the insiders' surplus, so the maximum surplus loss was 50%. Outsiders were given a table describing their payoffs as a function of the size of the insider surplus and their individual take from the surplus. I imposed this constraint to avoid the possibility that insiders would produce no surplus in order to avoid paying a significant amount of sanctions to deter theft. This also enables us to consider marginal deterrence, i.e. the use of sanctions to deter some but not all theft, similar to De Geest et al. (2017).

When outsiders finished making their decisions all subjects learned their initial payoffs. Insiders were then shown the outsiders' decisions and resulting payoffs. With this information, insiders could individually choose to levy costly sanctions, "Deductions", on each outsider and reduce his initial payoff to at most zero. I used the 1:3 sanctions technology introduced by Gächter and Fehr (2000) and that is now standard in the literature on experimental social dilemmas with peer-punishment. The amount an insider could spend on sanctions was constrained by his initial payoff. Moreover, punishment could not reduce an outsider's payoff below zero. After all sanctioning decisions were made, insiders and outsiders were informed their final payoffs for that round (initial payoffs less sanctions sent or received), as well as their cumulative payoffs across rounds.

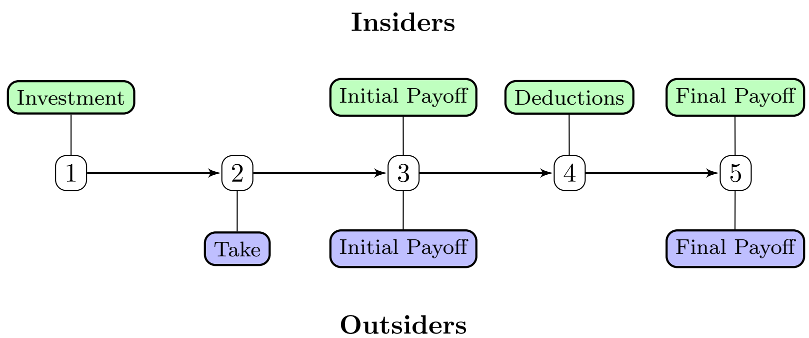

Figure 2.3: Timeline of stages in the theft treatments

A total of 120 subjects were recruited from the undergraduate population at UMass-Amherst. After subjects were assigned to their groups and assigned their identification numbers, an experimenter read the instructions aloud while the subjects followed along with their own copy. Subjects were paid \$5 for agreeing to participate and showing up on time and then earned additional money in the experiment. Subjects earned experimental dollars (\$ED) that were converted to US dollars at the pre-announced exchange rate of \$1 ED = \$0.01 US as in Kingsley and Liu (2014). Subjects were paid their combined earnings in cash at the end of the session. Earnings ranged between \$10.72 and \$22.75, with a mean of \$17.24 (σ= \$3.07). Sessions lasted approximately 90 minutes. The experiment was conducted using z-Tree (Fischbacher, 2007).

2.4 Results

Our results are divided into three subsections. I first look at insider cooperation in terms of contributions (PG) and appropriations (CPR) to and from the group account, which I hereon refer to simply as allocations. Next I examine theft by outsiders and look at efforts by insiders to deter them. Then I discuss our findings.

2.4.1 Group account contributions

Table 2.3 shows average insider allocations and cooperation indices across the four treatments. Similar to Kingsley and Liu (2014), I calculate a cooperation index that permits comparison of cooperation across externality settings. The index compresses observations in both settings relative to the symmetric social optimum (25) and the Nash CPR (40) and Nash PG (10) into the range [0,1]. The index is calculated as $1-(g_{hi}/e_i)$ for the CPR setting and g_{hi}/e_i for the PG setting. The value of the index for the social optimum is 0.5 and for the Nash CPR and PG is 0.2.

Table 2.3: Average insider allocations and cooperation. Standard deviations in parentheses.

	CPR, No theft	CPR, Theft	PG, Theft	PG, Theft
Allocation	36.17	32.82	17.39	16.21
	(11.62)	(11.95)	(12.91)	(13.00)
Cooperation index	0.28	0.34	0.35	0.32
	(0.23)	(0.24)	(0.26)	(0.26)
N	360	360	360	360

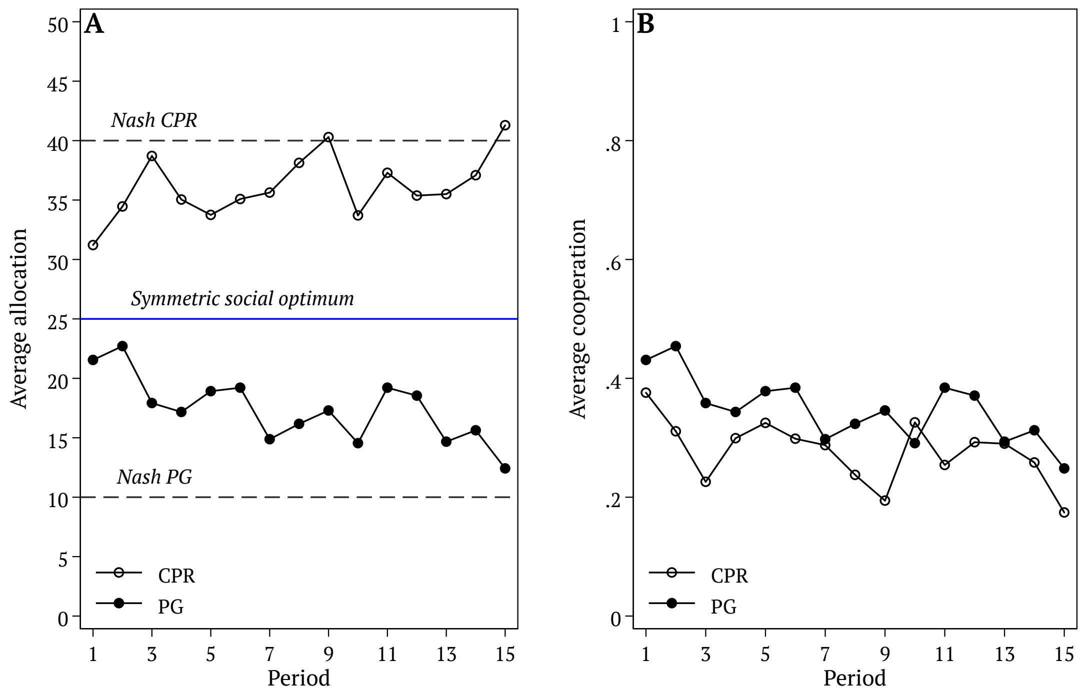

Figure 2.4: Average insider allocations and cooperation in PG and CPR no-theft treatements.

2.4.1.1 Replication of Kingsley and Liu (2014)

Similar to Kingsley and Liu (2014), I find a significant difference in allocations between the CPR and PG treatments without theft (Wilcoxon Rank-Sum test: $z = 2.88$, $p = 0.00$).[5] Figure 2.4 shows average group allocation and cooperation over fifteen periods. Like Kingsley and Liu (2014) I also see average group allocations in both the CPR and PG settings start near the social optimum and trend towards the respective Nash equilibria over time.

Unlike the authors, I find that cooperation is higher in the PG treatment and that the difference is significant (Wilcoxon Rank-Sum test: $z = -1.92$, $p = 0.05$). However, our results agree when I recreate their regressions of the cooperation in-

[5] I use the group average as the unit of observation.

dex. These results are reported in Table 2.4. Accounting for repeated observations within each subject using a random effects procedure, in the first model I regress the cooperation index on Period, Public (an indicator for the PG treatment), and the interaction Public $\times$ Period. The coefficients on Period and Public $\times$ Period are negative capturing the decrease in cooperation over time in both treatments that I observe in panel B of 2.4. The second model includes a vector of controls (Age, Gender and GPA) while the third model accounts for group fixed effects. Note the insignificant effects of Public on cooperation in models (2) and (3). Thus, I conclude that our control treatments, {CPR No Theft, PG No Theft}, recreated the main findings of Kingsley and Liu (2014) and Apesteguia and Maier-Rigaud (2006) who also found little evidence of a systematic difference in cooperation between the PG and CPR settings.

Table 2.4: Estimating Cooperation without Theft: Recreation of Table 4 in Kingsley and Liu (2014).

	(1)	(2)	(3)
Period	-0.006**	-0.006**	-0.006**
	(0.00)	(0.00)	(0.00)
Public	0.093*	0.074	0.063
	(0.05)	(0.05)	(0.11)
Public x Period	-0.003	-0.003	-0.003
	(0.00)	(0.00)	(0.00)
Age		0.012	0.021
		(0.01)	(0.02)
Gender		-0.051	-0.059
		(0.04)	(0.05)
GPA		0.011	0.032
		(0.04)	(0.05)
Constant	0.325***	0.126	-0.136
	(0.03)	(0.32)	(0.38)
Group fixed effects	No	No	Yes
N	720	720	720
R-squared overall	0.038	0.049	0.086
Rho	0.229	0.234	0.258

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

2.4.1.2 Cooperation in the presence of theft

I now turn to our theft treatments. Our main goal is to examine whether the outside threat increases or decreases cooperation in each setting. Recall that our initial expectation is that the outside threat would not produce any systematic differences in allocations or cooperation across the PG and CPR settings.

Figure 2.5 shows average insider allocations and the cooperation index in both treatments with and without outsider theft. Panel A shows average allocations across treatments, and Panels B and C show average levels of cooperation for the CPR and PG settings, respectively. Average allocations increase over time in the CPR treatment and decrease in the PG treatment.

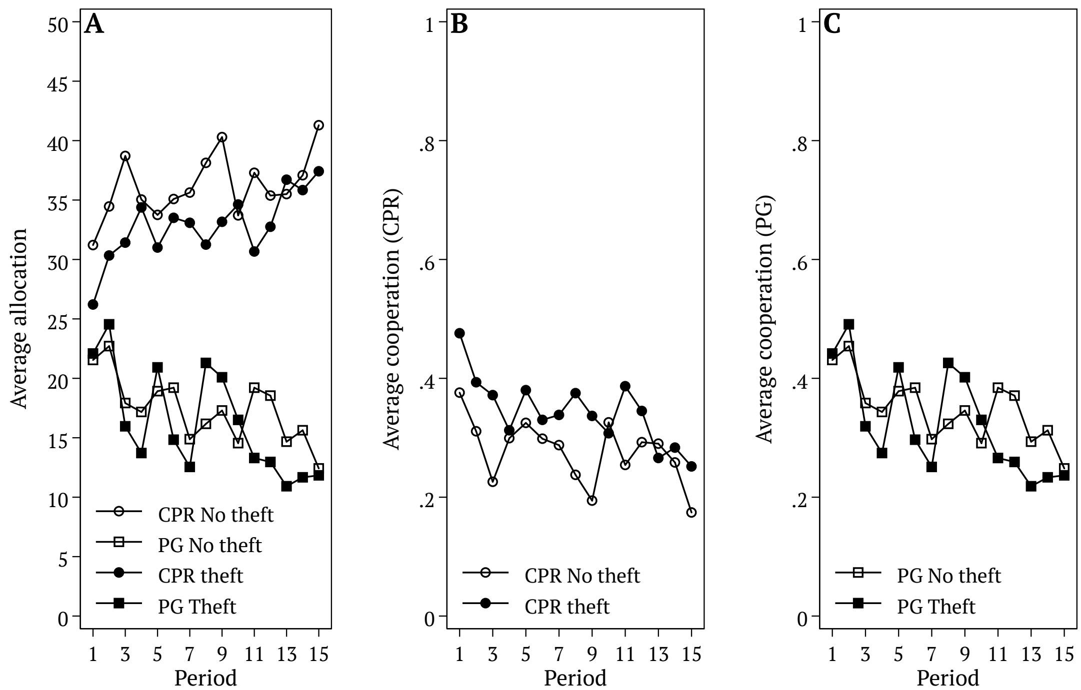

Figure 2.5: Average insider allocations and cooperation in PG and CPR no-theft and theft treatments.

Like in our controls, I find a significant difference in allocations between the CPR and PG treatments (Wilcoxon Rank-Sum test: $z = 2.89$, $p = 0.00$). To see if there was an effect of the outside threat, I tested for differences within treatments. I find a significant decrease in average allocation in the CPR setting (Wilcoxon Rank-Sum test: $z = 2.89$, $p = 0.00$). Table 2.3 shows that average cooperation increased in the CPR setting, and I find this increase to be significant (Wilcoxon Rank-Sum test: $z = 2.89$, $p = 0.00$).[6] Average allocation and cooperation decreased in the PG setting, but the differences are not significant (Wilcoxon Rank-Sum test for both

[6]The test results are similar because the cooperation index simply rescales allocations.

tests: $z = 1.21$, $p = 0.26$).[7] However, average allocations in the CPR setting are still much higher than the deterrence and non-deterrence social optima ($G_i^{S-,D}$ and $G_i^{S-,ND}$). While the external threat improves cooperation on allocation in the CPR setting, it does not push it to the socially optimal level.

Table 2.5 presents results from a random effects GLS regression on cooperation within each treatment and provides further statistical support for Panel A in 2.5. I estimate the CPR and PG settings separately and estimate multiple models for each setting to ensure robustness. In each model, Theft is an indicator variable for the treatment featuring an outside threat. The full specification is

$$\text{Cooperation}_{hit} = \alpha + \gamma_1 \text{Period}_t + \gamma_2 \text{Theft} + \gamma_3 (\text{Period}_t \times \text{Theft}) + \mathbf{X}'_{hi}\beta + \eta_j + \epsilon_{hit} \quad (2.18)$$

where $\mathbf{X}'_{hi}$ is the vector of time-invariant controls for insider hi, η_j are the group fixed effects ($j = \{1, \ldots, 6\}$) and ϵ_{hit} is a composite error term composed of a subject-specific term μ_{hi} and idiosyncratic error ε_{hit}. Our main interest is in the sign of γ_2 , the treatment effect. A positive sign for the Theft coefficient in each setting would indicate that the outside threat is associated with increased cooperation.

Table 2.5 reports our results. In the CPR setting the coefficient for Theft is positive and significant across all specifications, suggesting that the outside threat systematically increased cooperation. However, it did not reverse the decline in cooperation over time, as indicated by the negative (but insignificant) coefficient on the interaction of the treatment variable and time (Theft $\times$ Period).[8]

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[7] These findings are robust to linear random effects regression models that control for the dependence of individual decisions within and across time periods. The results of these regressions are reported in Table B.1 in the appendix.

[8] I used the same specification to estimate the cooperation index across treatments and found similar results. This is to be expected since the cooperation index simply rescales group allocation decisions.

Table 2.5: Estimating the cooperation index with and without an outside threat across treatments

	CPR			PG			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Period	-0.006*	-0.006*	-0.006*	-0.009***	-0.009***	-0.009***	-0.008***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Theft	0.095**	0.091**	0.129*	0.014	-0.001	0.161***	0.018
	(0.04)	(0.05)	(0.08)	(0.04)	(0.04)	(0.04)	(0.03)
Theft x Period	-0.003	-0.003	-0.003	-0.005	-0.005	-0.005	-0.005
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Age		0.009	0.011		-0.008	-0.008	-0.018***
		(0.02)	(0.02)		(0.01)	(0.01)	(0.01)
Gender		-0.038	-0.047		0.041	0.041	0.035
		(0.04)	(0.05)		(0.04)	(0.05)	(0.05)
GPA		0.005	0.006		-0.027	-0.020	-0.034
		(0.04)	(0.05)		(0.02)	(0.03)	(0.03)
Constant	0.230***	0.101	0.009	0.404***	0.630***	0.328	0.883***
	(0.07)	(0.39)	(0.61)	(0.05)	(0.24)	(0.31)	(0.16)
Group fixed effects	No	No	Yes	No	No	Yes	Yes
N	720	720	720	720	720	720	660
R-squared overall	0.041	0.050	0.057	0.038	0.051	0.081	0.082
Rho	0.205	0.211	0.262	0.266	0.272	0.309	0.265

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The estimated results for the PG setting are decidedly mixed. The effect of the outside threat in the PG setting is not robust – the coefficient on Threat changes sign and magnitude across specifications. Including group fixed effects (model (6)) leads to a highly significant and positive treatment effect. Since the treatment effect is identified from within-group variation, this might suggest that differences between groups are driving the result.

Consequently, I explore the variation in cooperation and allocations across groups and treatments in more detail. Figure 2.6 shows the average cooperation index per group across all periods, alongside the pooled mean (i.e. the mean of all groups in a setting and treatment) in red. The figure shows that while groups in the CPR×Theft treatment are tightly packed around the treatment mean, the average cooperation index in one group in the PG×Theft treatment was much smaller than the other groups. Excluding this group extinguishes the significant treatment effect of Theft in the PG setting (model (7) of Table 2.5). In addition, I tested for differences in within-group variation in allocations using the Levene and Brown-Forsythe tests of equal variances and report the results in Table 2.6.[9] The null hypothesis is that within-group variation in individual allocations are equal across treatments; a high F-statistic indicates a rejection of the null hypothesis. Our results show that within-group variation in allocations is higher in the PG setting than the CPR setting, both with theft and without theft. In addition, the presence of theft increases within-group variation in allocations in both the PG and CPR settings. [10]

[9] The Levene test uses the group mean to calculate spread within a group while the Brown-Forsythe test uses the group median. Both tests are approximately F-distributed. I ran both tests to ensure robustness of our results.

[10] I cannot compare these findings to Apesteguia and Maier-Rigaud (2006) or Kingsley and Liu (2014) as neither paper provides an analysis of dispersion at the group level. However, Apesteguia and Maier-Rigaud (2006) report standard deviations of individual decisions that indicate more dispersion in the PG setting than the CPR setting, similar to our results. Moreover, they find a significant difference between the distribution of decisions in the PG game and the truncated distribution of the CPR game (i.e. $e_i - x_i$ where e_i is the subject i's endowment and x_i is her decision).

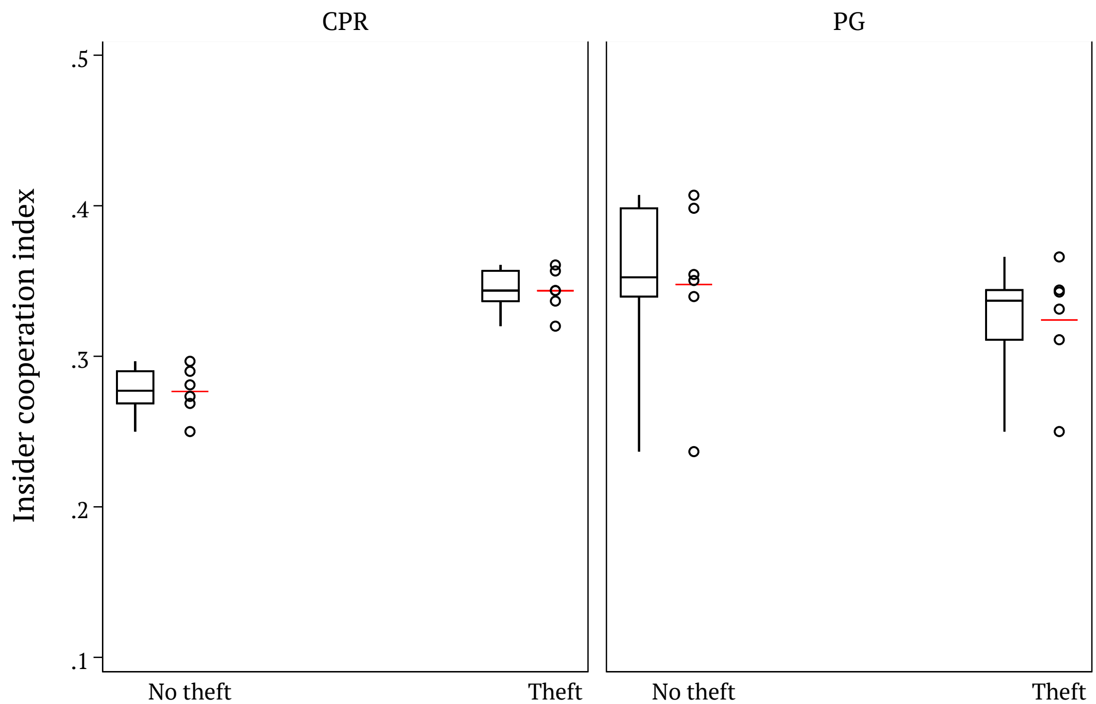


Figure 2.6: Average insider cooperation index by group. Each dot represents a group. The pooled mean is marked by the red lines.

Table 2.6: Tests of differences in the within-group variation of individual allocation decisions.

	CPR		PG	
	No Theft	Theft	No Theft	Theft
Levene's F statistic	2.340	7.935	10.049	15.554
Levene's p-value	0.041	0.000	0.000	0.000
Brown and Forsythe's F statistic (median)	1.760	4.463	7.126	8.282
Brown and Forsythe's p-value	0.120	0.001	0.000	0.000

Finally, in light of the finding of more cooperation in the CPR setting with theft, I looked at the distributions of group account allocations in each setting and treatment. Specifically, I calculated the difference in the kernel density estimates for each strategy across treatments. The results are shown in Figure 2.7. I observe a higher density of Nash behavior among insiders in the PG settings (10 unit investment per subject) than in the CPR setting (40 unit investment per subject) when an outside threat is in play. However, cooperative decisions in the CPR setting are more common in the theft treatment than in the PG setting (25 unit investment per subject in both settings). In the CPR setting there are 20% fewer individual decisions at or above the 40 units in the theft treatment versus the no-theft treatment, while in the PG setting there are about 19% more decisions at or below 10 units in the theft treatment.

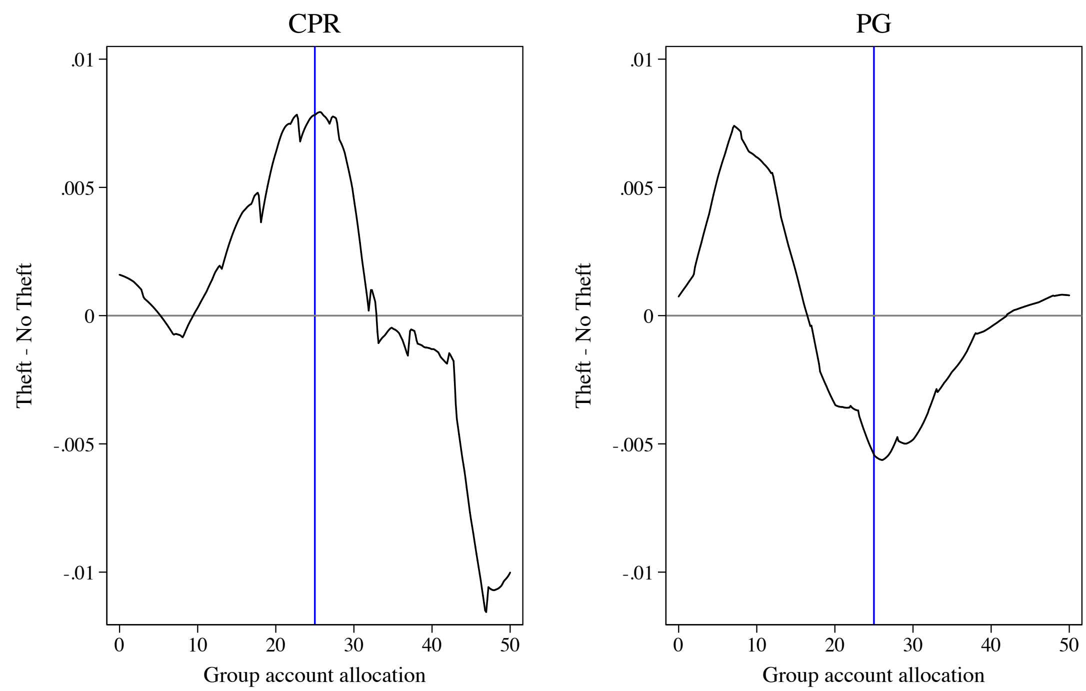


Figure 2.7: Differences in the kernel density estimates for each group account allocation in the theft and no-theft treatments for the CPR and PG settings. A positive density for a given strategy indicates it was more frequently chosen in the theft treatment. The blue line indicates the symmetric social optimum g^S. For the CPR (PG) figure, decisions to the left (right) are more (less) cooperative.

2.4.2 Theft & deterrence

Allocations to the group account is just one dimension on which insiders could cooperate. Insiders could also cooperate on deterrence, a second-order public good.

It is possible that insiders might produce less surplus than is socially optimal but still work together to deter outsiders. The main question in this section is how insiders coordinated on deterrence.

Figure 2.8 summarizes data from the Theft treatments. Panel A shows average gross surplus in experimental dollars across treatments and Panel B shows average surplus net of theft. Panel C shows how much surplus was, on average, taken by outsiders, expressed as a percent of the total surplus. (Recall that each outsider in the experiment were restricted to taking up to 25% of the surplus, so the maximum possible surplus loss was 50%). Finally, Panel D shows average insider deterrence, calculated as the total value of sanctions divided by total outsider payoffs from theft. Recall that to fully deter the outsiders, the insiders would have had to commit to removing 100% of each outsider's gain from theft.

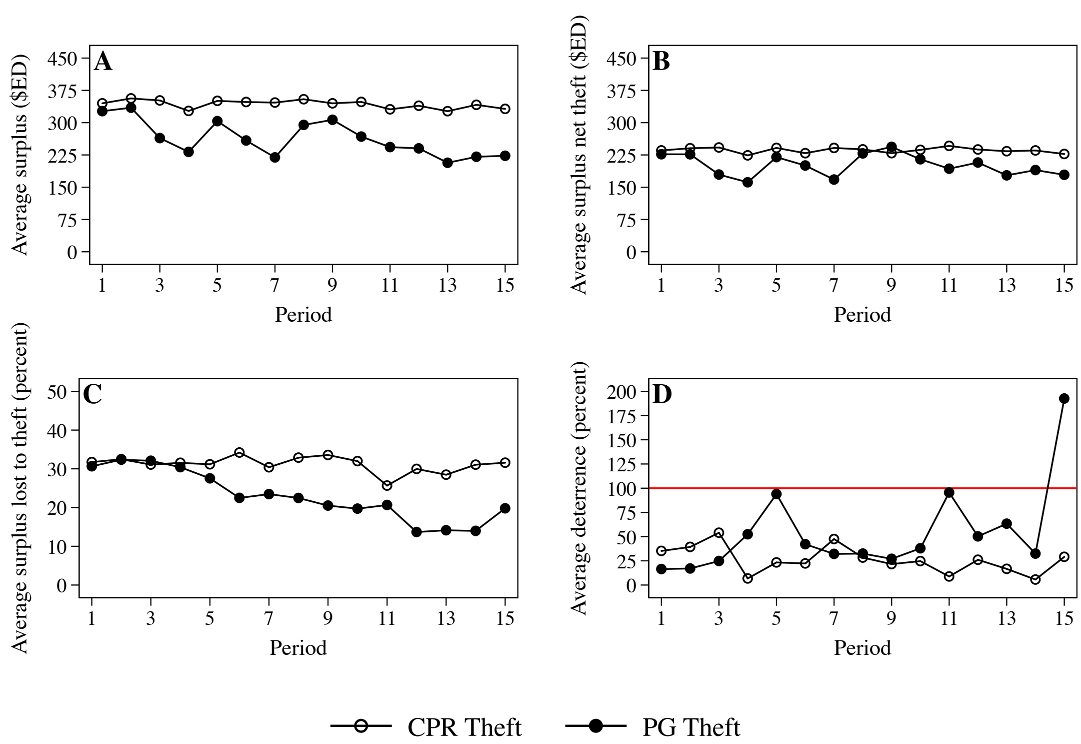


Figure 2.8: Panel A: Average surplus. Panel B: average surplus net of theft. Panel C: average surplus loss to outsider theft. Panel D: Average deterrence. The red line indicates 100% of theft payoffs eliminated.

Consistent with the higher levels of cooperation I observe in the CPR setting, average gross surplus is significantly higher in the CPR setting than in the PG setting (Wilcoxon Rank-Sum test: $z = 21.17$, $p = 0.00$). However, surplus loss from outside theft was higher in the CPR setting (Wilcoxon Rank-Sum test: $z = 11.68$, $p = 0.00$). Surplus net of theft is significantly higher in the CPR setting (Wilcoxon Rank-Sum test: $z = 7.31$, $p = 0.00$). Thus, the value of allocation cooperation is significantly higher in the CPR setting despite higher levels of theft. This is one of the main results of our study.

The higher levels of theft in the CPR setting can be partly be explained by lower levels of deterrence. Indeed, Panel D of 2.8 shows greater deterrence in the PG setting, and I find this difference to be significant (Wilcoxon Rank-Sum test: $z = -6.22$, $p = 0.00$). In addition, more insiders contributed sanctions in the PG setting (56% of the insiders in the PG setting versus 44% in the CPR setting). Note that this does not necessarily indicate that insiders in the PG setting levied higher sanctions than insiders in the CPR setting. For starters, it cost less to deter outsiders in the PG setting because average surplus was smaller. While there are more sanctioning events in the PG setting, the average of non-zero sanctions is higher in the CPR setting than in the PG setting (11.60 and 7.3, respectively), and the difference is significant (Two-sample t-test of means: $p = 0.01$.) Moreover, outsiders were sanctioned at a similar rate in both settings: outsiders were sanctioned approximately 49% of the time in the CPR setting and 56% of the time in the PG setting.

Panel C of 2.8 shows clearly that insiders in both setting were not able to deter outsider theft. Panel D indicates why: insiders on both settings could not coordinate deterrence well enough to consistently remove all of the gains from theft.

To better understand the decision to sanction in each treatment, I estimate a hurdle model that first estimates the binary decision to sanction and then, if the hurdle is passed, the magnitude of the sanction.[11] Similar to other experiments with some form of monetary punishment, the sanctions data from our experiment are zero-inflated, meaning most sanctioning events were zero (Chaudhuri, 2011). Our experimental design permits consideration of zero sanctions as explicit choices. In the Deductions stage, insiders had to enter a number of deduction points for each outsider. They were permitted not to sanction, but rather than simply click through the stage, they had to enter zero and click a button that showed them the cost of their decision. It is therefore reasonable to assume that zero sanctions were explicit choices.[12]

I first estimate the binary decision to sanction using a probit model. Then, I estimate the expected size of sanctions, conditional on a sanctioning event, using a zero-truncated negative binomial model. [13] Our full specification is

$$\begin{aligned} Pr(\text{sanctions}_{hit} > 0) &= \Phi(\alpha_1 + \gamma_{11}\text{Public} + \gamma_{12}\text{cooperate}_{hit} + \gamma_{13}\text{sanctions}_{hi,t-1} \\ &\quad + \mathbf{X}'_{jt}\delta_1 + \mathbf{X}'_{hi}\beta_1) \\ \mathbb{E}[\text{sanctions}_{hit}|\text{sanctions}_{hit} > 0] &= \exp(\alpha_2 + \gamma_{21}\text{Public} + \gamma_{22}\text{cooperate}_{hit} + \gamma_{23}\text{sanctions}_{hi,t-1} \\ &\quad + \mathbf{X}'_{jt}\delta_2 + \mathbf{X}'_{hi}\beta_2), \end{aligned} \tag{2.19}$$

where Public is an indicator variable for the PG treatment, cooperate_{hit} is the subject's cooperation index value in a given period, $\text{sanctions}_{hi,t-1}$ is the amount of sanc-

[11]Kingsley (2015) also uses a hurdle model to study peer-punishment decisions in the same payoff equivalent CPR and PG games as Kingsley and Liu (2014).

[12]This is an important point. Alternatively, if subjects could either choose zero sanctions or skip the stage altogether, then I would have to ensure that observations of zero sanctions were not generated by two different processes. In that case it would be more appropriate to choose a procedure that distinguishes "types" of zeros, such as zero-modified count models (e.g. zero-inflated negative binomial regression) where the expected value is calculated as a convex combination of the two zero-generating processes.

[13]I use a count data method because deduction points were integer-valued and bounded below at zero and to account for overdispersion in sanctions. In both the CPR and PG setting the standard deviation of sanctions is nearly four times greater than the mean. The mean (standard deviation) for all sanctions, including zeros, was 2.35 (7.69) in the CPR and 2.49 (8.24) in the PG setting.

tions assigned by the subject in the previous period $\mathbf{X}'_{jt}$ is a vector of group controls that varied with time including period and individual surplus loss, $\mathbf{X}'_{hi}$ is a vector of individual time-invariant controls including age, gender and GPA. To control for within-group correlation, standard errors are clustered at the group level.

Our goal is to look for systematic differences in sanctions across treatments. As such, I am interested in the signs of $\hat{\gamma}_{11}$ and $\hat{\gamma}_{21}$. I am also interested in whether cooperation on one margin is associated with cooperation on the other – if insiders produce more surplus, do they also supply more sanctions? I am able to investigate this because insiders chose levels of cooperation sequentially. If suplus creation does lead to more surplus protection, then I would expect the estimates $\hat{\gamma}_{12}$ and $\hat{\gamma}_{22}$ to be positive and significant.

Table 3.2 shows our results, grouped by Selection (the estimated probability of a sanctioning event, models (1) and (2)) and Count (the estimated sanction value conditional on it not being zero, models (3) and (4)). Models (1) and (2) and (3) and (4) show the estimated coefficients and average marginal effects, respectively. Our main finding is that the likelihood of a sanction is significantly higher in the PG setting (Public in models (1) and (2)), but there is no significant difference in sanction size (Public in models (3) and (4)). Individual surplus and lagged sanctions are positive and significant in both the PG and CPR settings. This suggests sanctions wer mainly driven by losses from surplus, controlling for surplus size, and whether the subject had sanctioned before, controlling for any systematic time effects. Consistent with what I see in Figure 2.8 Panel D, there is no significant time trend in either setting.

In addition, I find no significant relationship between cooperation and deterrence in the decision to sanction, but I find a significant and negative relationship on the levels of sanctions, suggesting that sanction size is negatively correlated with cooperation on allocation. Those who cooperated more on allocations were not more likely to levy sanctions on outsiders, but when they did impose sanctions the sanctions were lower. This result may be due to the simple fact that insiders who cooperated less had higher initial payoffs and thus had more disposable income for sanctions. However, it is possible that there was some implicit coordination among insiders that those who produced less of the surplus were responsible for more of the sanctions.

To further investigate cooperation and deterrence, I calculated the average marginal effects in both models, varying the cooperation index between zero and one in increments of 0.1 and holding all other variables at their means. The results for the selection and count models are shown in Panels A and B of Figure 2.9, respectively. The predicted curves of cooperation on $Pr(\text{sanctions} > 0)$ are flat in each treatment, consistent with the insignificant point estimate from our estimated model.

There is more activity in the predicted curves of cooperation on the expected size of sanctions. Both treatments exhibit a negative relationship between cooperation on group allocation and the number of sanctions at the agent level. Again, the negative slope is likely due to the fact that cooperation tended to yield lower initial payoffs and thus fewer sanctioning resources. Interestingly, the effect seems to be more prominent in the CPR treatment. However, the estimates are very noisy. I cannot not reject the null hypothesis of no meaningful connection between cooperation and deterrence.

Table 2.7: Estimating the supply of deterrence across treatments. (1) and (3) show the estimated coefficients. (4) and (6) show the average marginal effects.

	Selection		Count	
	(1)	(2)	(3)	(4)
Public	0.667***	0.193***	0.032	0.247
	(0.20)	(0.05)	(0.44)	(3.38)
Period	-0.009	-0.003	0.051	0.390
	(0.01)	(0.00)	(0.04)	(0.30)
Cooperation (Allocation)	-0.088	-0.025	-1.035**	-7.916**
	(0.25)	(0.07)	(0.46)	(3.66)
Sanctions in $t-1$	0.036*	0.010**	0.039**	0.302**
	(0.02)	(0.01)	(0.02)	(0.13)
Surplus Loss (Individual)	0.022***	0.006***	0.036**	0.276*
	(0.01)	(0.00)	(0.01)	(0.15)
Age	-0.123	-0.035*	0.125*	0.952*
	(0.08)	(0.02)	(0.07)	(0.58)
GPA	-0.205	-0.059	-0.119	-0.912
	(0.22)	(0.06)	(0.47)	(3.69)
Gender	0.129	0.037	-0.326	-2.572
	(0.22)	(0.06)	(0.20)	(1.73)
Constant	1.638		-1.277	
	(1.80)		(2.54)	
N	672	672	177	177
Wald Chi-squared	41.223***		94.059***	
Pseudo R-squared	0.117		0.029	

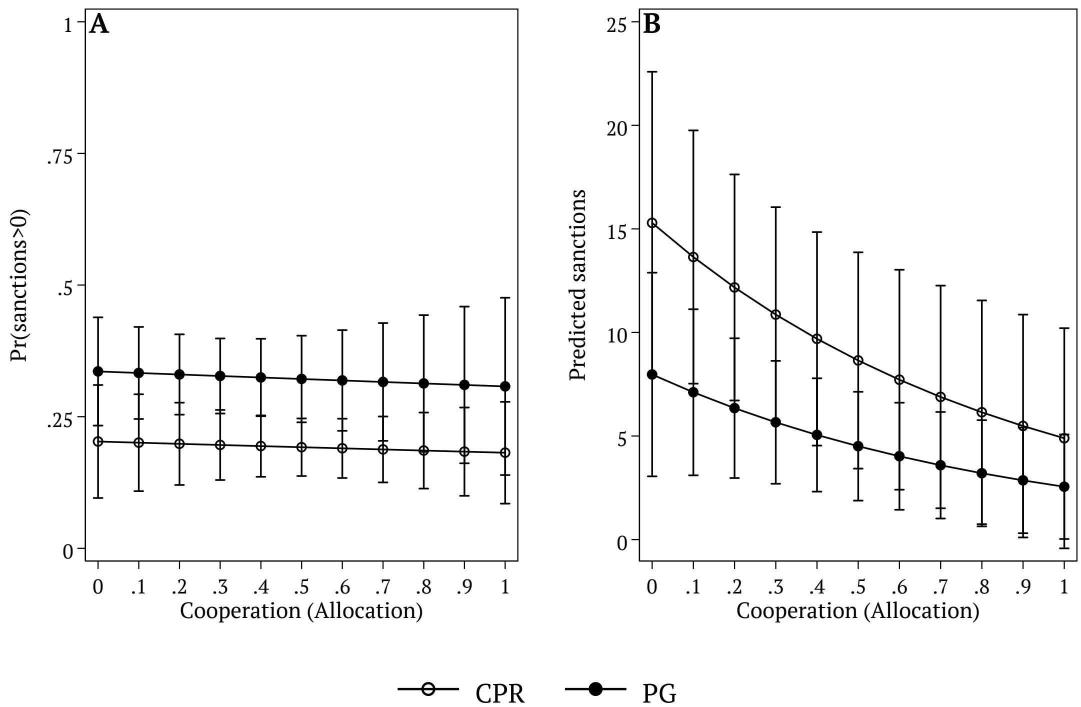


Figure 2.9: Marginal effects of allocation cooperation on predicted sanctions (estimates from Table 3.2).

2.4.3 Outsider response to sanctions

Finally, I investigate how outsiders responded to sanctions in each treatment by estimating the following random effects GLS model

$$\begin{aligned}\Delta\text{Theft} = \alpha + \gamma_1\text{Public} + \gamma_2\text{Sanctions}_{ho,t-1} + \gamma_3(\text{Public} \times \text{Sanctions}_{ho,t-1}) \\ + \mathbf{X}'_{jt}\delta + \mathbf{X}'_{ho}\beta + \eta_j + \epsilon_{hot}\end{aligned} \tag{2.20}$$

where ΔTheft is an outsider's change in theft between two periods (i.e. $\text{Theft}_{ho,t+1} - \text{Theft}_{ho,t}$), Public is the PG indicator, Sanctions_{hot} are the sanctions received by an outsider in period t, $\mathbf{X}'_{jt}$ are group controls including period and gross surplus in t, $\mathbf{X}'_{ho}$ are individual time-invariant controls including Age, Gender and GPA, η_j are group fixed effects, and ϵ_{hit} is a composite error term composed of a subject-specific term μ_{hi} and idiosyncratic error ε_{hit}. Standard errors are clustered by group to control for within-group correlation. In both treatments the majority of sanctions went to the outsider in each group who stole more of the surplus (62% in the CPR, 63% in the PG). I therefore estimated separate models for outsiders who stole more and outsiders who stole less or an equal amount.

Table 2.8 reports our results. The coefficient on Surplus Value is positive, indicating that an increase in surplus led to more theft, and the effect is stronger for outsiders who tended to steal more of the surplus. I find no systematic changes in theft in response to sanctions in either the main effect (Sanctions) or the interaction (Public $\times$ Sanctions). The treatment indicator is significant, indicating that theft declines more in the PG setting than in the CPR treatment. This is likely due to the fact that gross surplus decreased more in the PG setting. Altogether, I find no evidence that sanctions systematically deterred theft. Instead it appears that surplus size was the main determinant of changes in theft.

Table 2.8: Estimating outsider response to sanctions in each treatment. Separate models are estimated for outsiders who stole less or an equal amount of the surplus ("Lower or Equal Theft") versus outsiders who stole more ("Higher Theft").

	Lower or Equal Theft			Higher Theft		
	(1)	(2)	(3)	(4)	(5)	(6)
Public	-4.739	-4.820	-16.828***	-3.621	-1.930	-14.686*
	(7.09)	(7.27)	(1.82)	(7.86)	(7.42)	(7.83)
Sanctions	0.002	-0.001	-0.091	-0.306	-0.299	-0.500
	(0.15)	(0.15)	(0.16)	(0.39)	(0.41)	(0.39)
Public x Sanctions	-0.131	-0.130	-0.086	0.384	0.371	0.545
	(0.19)	(0.19)	(0.21)	(0.40)	(0.42)	(0.40)
Surplus	0.044**	0.044**	0.043**	0.105**	0.107**	0.102**
	(0.02)	(0.02)	(0.02)	(0.04)	(0.04)	(0.04)
Period	-1.047**	-1.038**	-1.181***	0.020	0.016	0.107
	(0.47)	(0.48)	(0.45)	(0.38)	(0.39)	(0.41)
Age		-0.416	-1.359		-1.201	-1.357
		(0.69)	(0.91)		(0.88)	(2.14)
GPA		-2.888	-5.442*		-3.830	-5.723
		(2.84)	(3.27)		(4.86)	(6.72)
Gender		-4.456	-0.015		-1.279	8.536
		(5.36)	(4.81)		(6.08)	(5.39)
Constant	22.958*	48.680***	81.465***	-1.772	36.163*	36.354**
	(12.03)	(16.08)	(19.35)	(16.77)	(18.60)	(18.53)
Group fixed effects	No	No	Yes	No	No	Yes
N	185	185	185	151	151	151
R-squared overall	0.198	0.223	0.502	0.196	0.237	0.578
Rho	0.369	0.385	0.000	0.342	0.381	0.000

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

2.5 Conclusion

Groups of individuals often cooperate to produce shared benefits, yet there are many scenarios in which they must collectively deter an outside threat from appropriating this surplus. Cooperation in groups is thought to be sensitive to how actions are framed, or more specifically, whether individual actions impose positive or negative externalities on other group members. This so-called "cooperation divergence", in which average levels of cooperation and first and second-order beliefs about cooperation, are thought to be higher in public goods games than in common-pool resource games. However, this result has been challenged by social dilemma researchers in recent years. While there has been a lot of focus on cooperation to create surplus, little to no attention has been paid to group efforts to defend their surplus.

This paper contributes to the literature by developing a theoretical model and an experimental framework of payoff equivalent, strategically symmetric common-pool resource and public goods games in which a group of individuals create surplus (e.g. provide a public good or conserve a common-pool resource) and deter theft by outsiders. After replicating Kingsley and Liu (2014) in the absence of theft, I introduce treatments in which individual outsiders can steal the surplus created by insiders, but insiders can deter the outsiders with sanctions. Our results suggest that in the absence of theft, there does not appear to be a significant difference in cooperation in the PG and CPR settings as in Kingsley and Liu (2014). However, the presence of theft led CPR users to cooperate more in terms of group allocations, while theft had no effect on average cooperation by PG providers. However, losses to theft were significantly higher in the CPR setting. Despite this, CPR users produced greater surplus net of theft than PG providers. I also find that variation in individual contributions to group accounts is higher in both PG treatments than in the CPR treatments, and the introduction of theft increases the variation of individual contributions in both settings.

Since I am interested in how an outside threat serves to coordinate group behavior, insiders in our experiment had no explicit coordination mechanisms like communication, voting, or feedback on sanction decisions by other group members. An outside threat can serve as a focal point to coordinate group behavior, but the threat can also increases strategic uncertainty. Insiders, already lacking a way to coordinate allocations to the group account, must now contend with the added difficulty of coordinating deterrence. This may explain why I observe increased variation in group allocations when I introduce theft in both CPR and PG settings. Examining the benefits of additional coordinating devices such as communication and feedback about individual allocation and deterrence decisions may be promising topics for future research.

CHAPTER 3

ENFORCEMENT NETWORKS IN SOCIAL DILEMMAS

3.1 Introduction

Cooperation in social dilemmas relies on institutions that curb self-interested behavior. One of the most studied institutions in economic experiments and evolutionary models is costly peer punishment – individuals sanctioning others at a cost to themselves.[1] Social dilemmas are characterized by a tension between group and individual incentives. Punishment comes in many flavors (e.g. monetary and non-monetary) but the intuition behind each one is similar. Punishment can help align group and individual incentives by establishing a cost to selfish behavior. So long as the cost of selfishness outweighs the benefits, cooperation is a mutual best-response.

However, establishing this cost is a challenge because it requires individuals to use punishment in a way that ensures it is a credible threat. Usually this means collectively signaling that bad behavior will be punished while good behavior will be rewarded. Once punishment is credible it need not be used, but the process of making it credible often causes substantial social costs.[2] Many studies using a variety of punishment institutions find the social costs of enforcement often negate the benefits from increased cooperation (Casari, 2005; Carpenter, 2007; Nikiforakis,

[1]For example, Ostrom et al. (1992); Gächter and Fehr (2000); Fehr and Gächter (2002); Masclet et al. (2003); Fehr and Fischbacher (2004a); Bowles and Gintis (2011), and many others. See Chaudhuri (2011) for a review.

[2]Many other studies examine centralized enforcement, in which group behavior is regulated by a third-party. Centralized enforcement tends to be more efficient than decentralized enforcement because subjects acknowledge the threat of punishment as credible in shorter time. See for example Fehr and Fischbacher (2004b); Almenberg et al. (2010); Andreoni and Gee (2012).

2008; Nikiforakis and Normann, 2008; Nikiforakis, 2010). Gächter et al. (2008) show that punishment increases cooperation in the short-run but only increases welfare in the long-run, consistent with group selection models of cooperation where non-cooperators are selected out of a population in evolutionary time (Choi and Bowles, 2007; Bowles and Gintis, 2011). Moreover, these long-run benefits of punishment hinge on environmental conditions. Gächter et al. (2008) consider a public goods game where subjects have perfect information about contributions. Ambrus and Greiner (2012) show that when information about contributions is imperfect, punishment does not improve welfare because it takes subjects longer to wield punishment effectively.

De Geest et al. (2017) point out that the problem of establishing a credible threat is related to the problem of coordinating enforcement responsibilities. Enforcement is a second-order public good, so individuals have an incentive to freeride on enforcement provided by others. Most studies focus on estimating average population effects by pooling data across independent groups. However, the messy process of subjects figuring who should punish who, when and how much requires closer attention to the dynamics within individual groups. This is consistent with evidence from the field. Among decentralized communities managing common-pool resources, enforcement responsibilities vary widely and with varying success (Ostrom, 1990; Chhatre and Agrawal, 2008; Stevens et al., 2015).

In the standard social dilemma experiment, a period of play consists of two decisions stages. First, subjects decide how much to contribute towards a public good or appropriate from a common-pool resource. After observing the decisions of their group members, and their own initial payoffs, subjects then choose how much punishment to levy. This second stage can be thought of as a graph, where vertices represent subjects and directed, weighted edges represent instances and values of peer punishment. These enforcement networks can then be summarized, on aggregate and for each time period, at the group and dyadic level. This allows the researcher to explore latent structures in the data that would otherwise remain hidden when testing only for average effects.

This chapter takes a network approach to study the evolution of decentralized enforcement in a social dilemma experiment. Using the data from De Geest et al. (2017), I recover the enforcement networks for each group in each period and summarize them using standard tools from network science. De Geest et al. (2017) use an experiment to study how uncoordinated groups can deter poaching of common-pool resources when monitoring of poachers is limited. Monitoring of poachers is varied across three treatments: not at all (Zero Monitoring), incomplete (Partial Monitoring), in which one poacher is randomly observed, and complete (Full Monitoring). Subjects managing and defending the resource can punish any poachers they observe. In addition, they could communicate with each other and punish each other in each treatment.[3] The authors find that perfect monitoring improved welfare, but across treatments, enforcement was on average ineffective at deterring outsiders and regulating insiders.

Our approach yields novel insights about decentralized enforcement. While enforcement varies widely across groups, it usually clusters around a subset of individuals. Senders of punishment (insiders) tend to appropriate less from the resource, receivers tend to appropriate more (insiders and outsiders). However, enforcement is often inefficiently distributed – sanctions that are better served on individuals with high appropriation often go to individuals with low appropriation instead. Moreover, I find clustering changes across treatments. Reciprocity in enforcement is higher when insiders cannot monitor outsiders. This captures the fact that insiders tend to trade-off regulating insiders and deterring outsiders. Estimating enforcement decisions with unique individual network indicators, I find different effects of sanction events (edge

[3]Other studies investigate how network structure impacts cooperation in public goods games. See for example Fatas et al. (2015); Carpenter et al. (2012); Elliott and Golub (2013); Wunder et al. (2013); Mantilla (2015); Leibbrandt et al. (2015).

formation) and sanction size (edge weight). While attention is often focused on the volume of punishment, our approach shows that efficient enforcement is also related to the number of binary sanction events and the number of subjects participating in enforcement.

The rest of this chapter proceeds as follows. Section 2 analyzes the enforcement networks in De Geest et al. (2017) in three parts. In the first part, network summary statistics are calculated for all groups and treatments.[4] In the second part, measures of network centrality are calculated for each individual in each period and then used to estimate models of enforcement decisions and efficiency. Finally, I explore stationary properties of groups by simulating a Markov process where states correspond to the structure of enforcement networks. Section 4 concludes and discusses how our approach can be used in other economic experiments that involve dyadic exchange.

3.2 Background

De Geest et al. (2017) recruited 96 subjects from the student population at the University of Massachusetts-Amherst to participate in a neutrally-framed common-pool resource experiment with poaching.[5] Subjects were randomly allocated across three treatments that varied the monitoring networks that governed the ability for insiders (subjects who managed the resource) to observe and punish outsiders (subjects who poached the resource). Insiders could perfectly monitor each other in each treatment. The experiments were programmed in zTree (Fischbacher, 2007).

Play proceeded for fifteen periods as follows in each treatment. In odd-numbered rounds, insiders could communicate with each other in the first stage via anonymous

[4]I used the `igraph` package in R to construct enforcement networks from the experiment data and calculate network measures (Csardi and Nepusz, 2006).

[5]A full description of the experiment, as well as the experiment instructions, are available in De Geest et al. (2017).

computer chat. This stage was skipped in even-numbered rounds. Next, insiders chose levels of appropriation from the common-pool resource, while outsiders chose levels of poaching. Both sets of subjects were then informed of their initial payoffs. In the following stage, insiders could then choose to levy punishment on other insiders and any outsiders they could observe according to the monitoring network (none in Zero Monitoring, one randomly chosen in Partial Monitoring, and all in Full Monitoring). Insiders could purchase one deduction point at a cost of one experimental dollar and assign it to a monitored subject to reduce their payoffs by three experimental dollars (i.e. Fehr-Gächter punishment). For those subjects the insiders could observe, they saw their appropriation or poaching decisions and initial payoffs in that round. Insider purchases of sanctions were constrained only by their initial payoffs. Once this round ended, subjects viewed their final payoffs, which were simply their initial payoffs net of the costs of sanctions assigned and/or received.

The enforcement stage can be cast as a problem of graph formation in which insiders seek the minimum configuration of edges and weights to control behavior. In each period, an enforcement network is realized from a hypergraph that defines the set of feasible edges between a fixed set of vertices (Berge and Minieka, 1973). For a given treatment let $G_k = (V, E, w)$ be the hypergraph of group k, where V is the set of vertices (subjects) indexed by insiders $i \in 1, \ldots, 5$ and outsiders $j \in 1, \ldots, 3$, E is the set of edges (sanctioning events) indexed by sanctions on insiders e_i and outsiders e_j, and w are the corresponding edge weights (sanction sizes). Figure 3.1 shows the hypergraph for a group in the Full Monitoring treatment. It follows that an enforcement network in period t is simply a realization of G_{kt} in T.

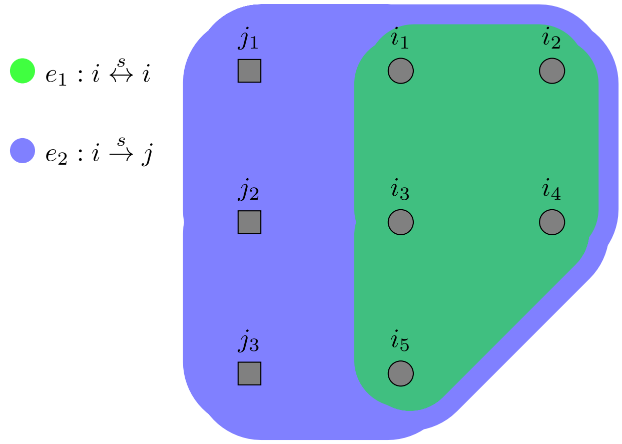


Figure 3.1: Hypergraph of a group in the Full Monitoring treatment.

Observing $G_{kt}\ \forall\ t \in T$ requires us to observe not just total sanctions by individuals in a given period, but their decisions with respect to each subject that could be sanctioned. In De Geest et al. (2017), sanctions were recorded by zTree using contracts tables that recorded each dyadic "exchange" between subjects. Specifically, a record was created for each instance that an insider assigned sanctions to another insider in her group or an outsider she monitored, including the size of the sanction. This allows us to observe both the aggregate enforcement networks for each group as well as the enforcement network in each period and calculate various network measures at the individual and group level.

3.3 Summarizing enforcement networks

3.3.1 Visual analysis

I begin with a graphical analysis of the aggregate enforcement networks. For each group in each treatment I sum E_k and w_k across all periods and plot them using the Fruchterman-Reingold force-directed algorithm, a standard approach to visualizing small networks (Fruchterman and Reingold, 1991). Insiders are denoted by circles while outsiders are denoted by squares. Vertex size is determined by a subject's total appropriation from the commons. In each group I calculate three standard network centrality measures to highlight the structure of the enforcement network.[6] Green nodes represent the subject with the highest out-degree score, meaning the subject provided the most sanctions among insiders. Blue nodes represent the subject with the highest in-degree score, the subject who received the most sanctions. Measures of degree centrality are among the simplest but they capture two important aspects of an enforcement network's structure: where are sanctions coming from and where are sanctions going. I also include a measure of eigenvector centrality that prioritizes subjects who are "connected" to influential subjects. Intuitively, this measure should correlate with out-degree centrality, since subjects who receive the most sanctions are receiving them subjects who provide the most sanctions.[7]

Finally, subjects with the highest eigenvector centrality are trimmed in red. Eigenvector centrality refers to a class of centrality measures that calculate the principle eigenvector for a specific eigenvalue problem of a network's adjacency matrix. The most common is Bonacich centrality, $\mathbf{Ax} = \lambda_1 \mathbf{x}$, where $\mathbf{Ax}$ is the adjacency matrix (the nxn matrix of weighted edges between subjects), x is the vector of eigenvector centralities and λ_1 is the eigenvector corresponding to the largest eigenvalue (i.e. the principal eigenvector). I use a modification of this measure known as PageRank that performs better for sparse, directed graphs by scaling the centrality measure by the subject's out-degree or provided sanctions, k_i^{out}. This avoids exaggerating the centrality of a subject simply because she is receiving sanctions by a single high-centrality subject.

[6]There are many measures of network "importance" or centrality. See Jackson et al. (2008) for a review.

[7]In the aggregate enforcement networks there is one only case where the highest out-degree subject is not the highest eigenvector central subject, Group 1 in Partial Monitoring. The leading out-degree subject (47 total sanctions, PageRank score of 0.18) has a more skewed distribution of received sanctions from each insider compared to the leading eigenvector centrality subject (43 total sanctions, PageRank score of 0.20).

Figures 3.2-3.4 show the aggregate enforcement networks for each group in Zero Monitoring, Partial Monitoring and Full Monitoring. The figures display interesting similarities and differences in structure across groups and treatments. The majority of sanctions to both insiders and outsiders tend to come from insiders who appropriate less of the resource, while most sanctions are sent to the high harvesters and/or poachers. However, this is not strictly true for all groups. In Zero Monitoring, Groups 3 and 4 saw the second-leading harvester become the leading punisher, though in all four groups the leading harvester bore the brunt of aggregate sanctions. Interestingly, in Group 2 in Partial Monitoring the same insider both received and assigned the most sanctions. Visual inspection also reveals the dispersion in graph density (the number of sanctioning events and their values) across groups and across treatments.

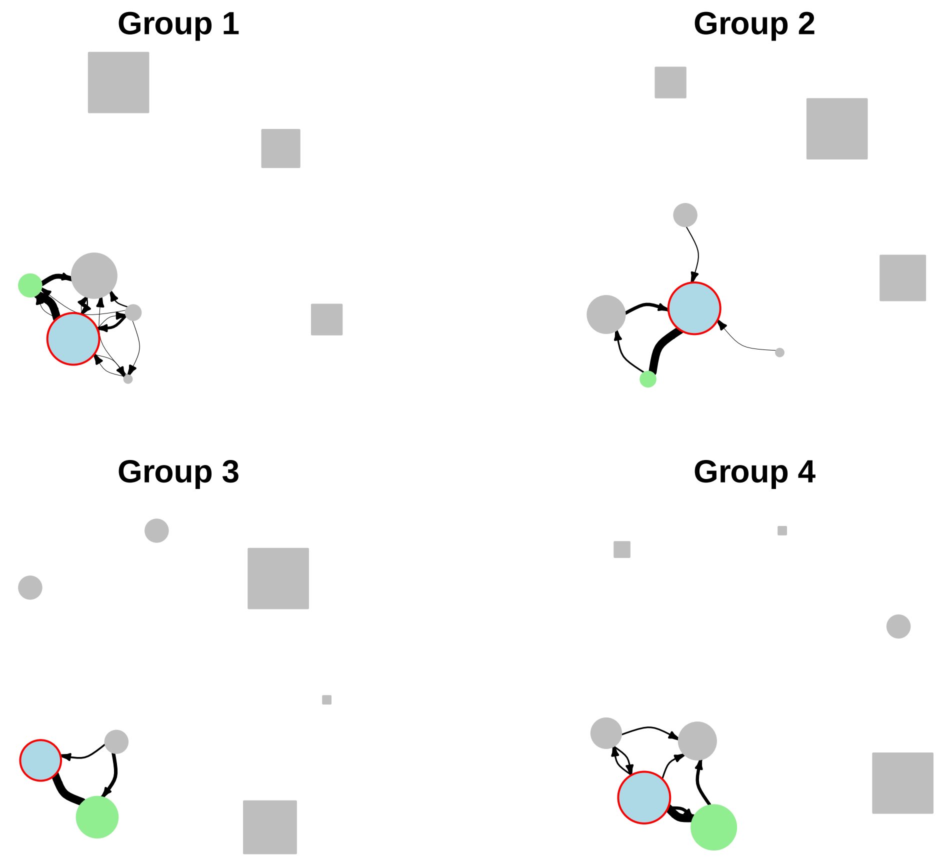


Figure 3.2: Aggregate group enforcement networks: Zero Monitoring. Insiders are round nodes, outsiders are square nodes. Node size represents a subject's aggregate level of harvest or poaching. Node color represents highest in-degree (blue) and out-degree (green) centrality. Red trim indicates the node with the highest eigenvector centrality. Directed edges indicate sanctions; edge size represents aggregate sanctions.

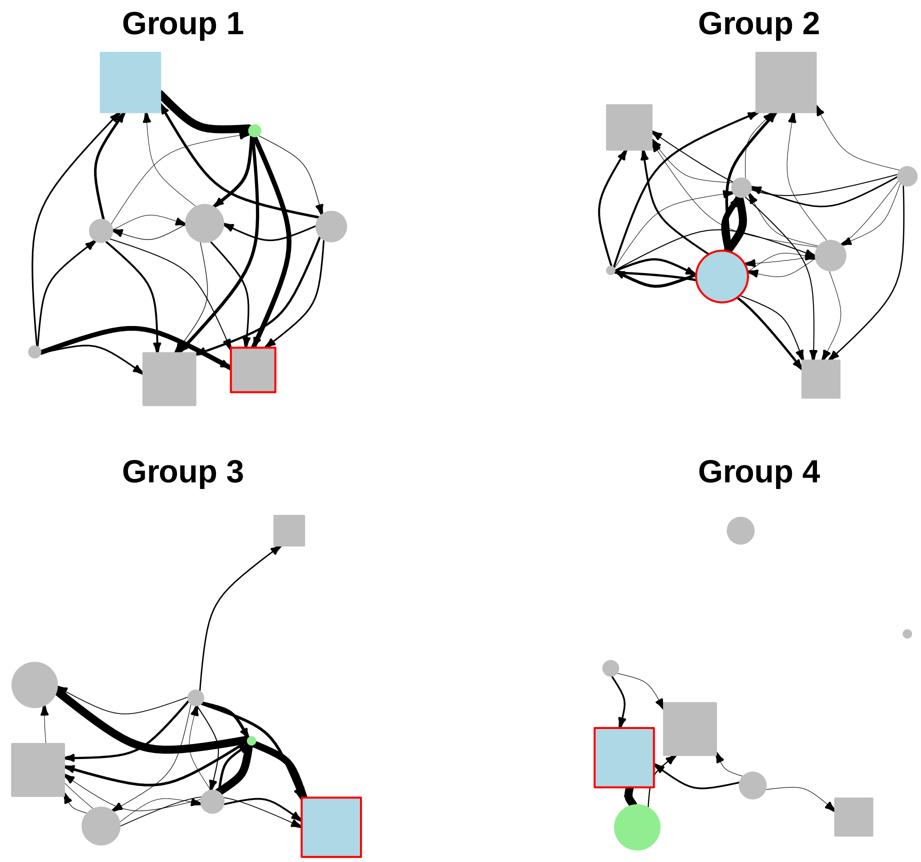


Figure 3.3: Aggregate group enforcement networks: Partial Monitoring. Insiders are round nodes, outsiders are square nodes. Node size represents a subject's aggregate level of harvest or poaching. Node color represents highest in-degree (blue) and out-degree (green) centrality. Red trim indicates the node with the highest eigenvector centrality. Directed edges indicate sanctions; edge size represents aggregate sanctions.

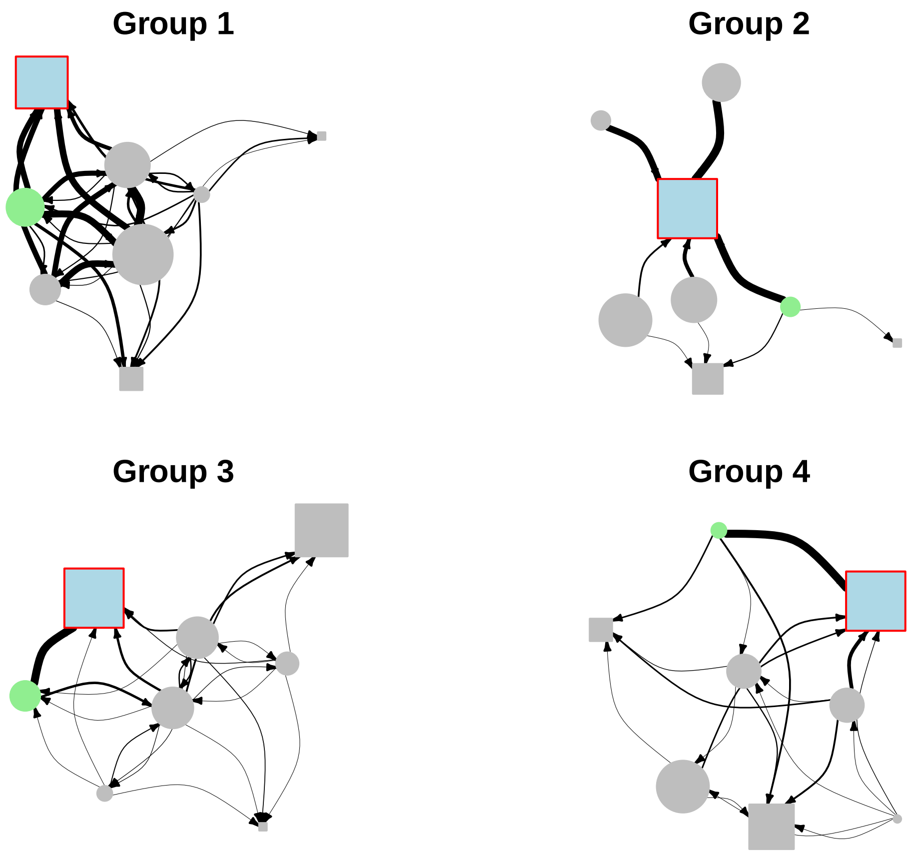


Figure 3.4: Aggregate group enforcement networks: Full Monitoring. Insiders are round nodes, outsiders are square nodes. Node size represents a subject's aggregate level of harvest or poaching. Node color represents highest in-degree (blue) and out-degree (green) centrality. Red trim indicates the node with the highest eigenvector centrality. Directed edges indicate sanctions; edge size represents aggregate sanctions.

I zoom in on each treatment by looking at the number of sanctioning events and their sizes (i.e. number of edges and edge weights) for each enforcement network in each time period in each treatment, i.e. $G^{\theta}_{kt}\ \forall\, t \in T$ where $\theta \in \{\text{Zero},\ \text{Partial},\ \text{Full}\}$ indexes the treatment. These statistics are then pooled so I can plot their distributions. The results for the aggregate enforcement networks (sanctions on insiders and outsiders) are shown in Panels A and B of Figure 3.5, while the results for insider enforcement networks (sanctions between insiders) are shown in Panels C and D.

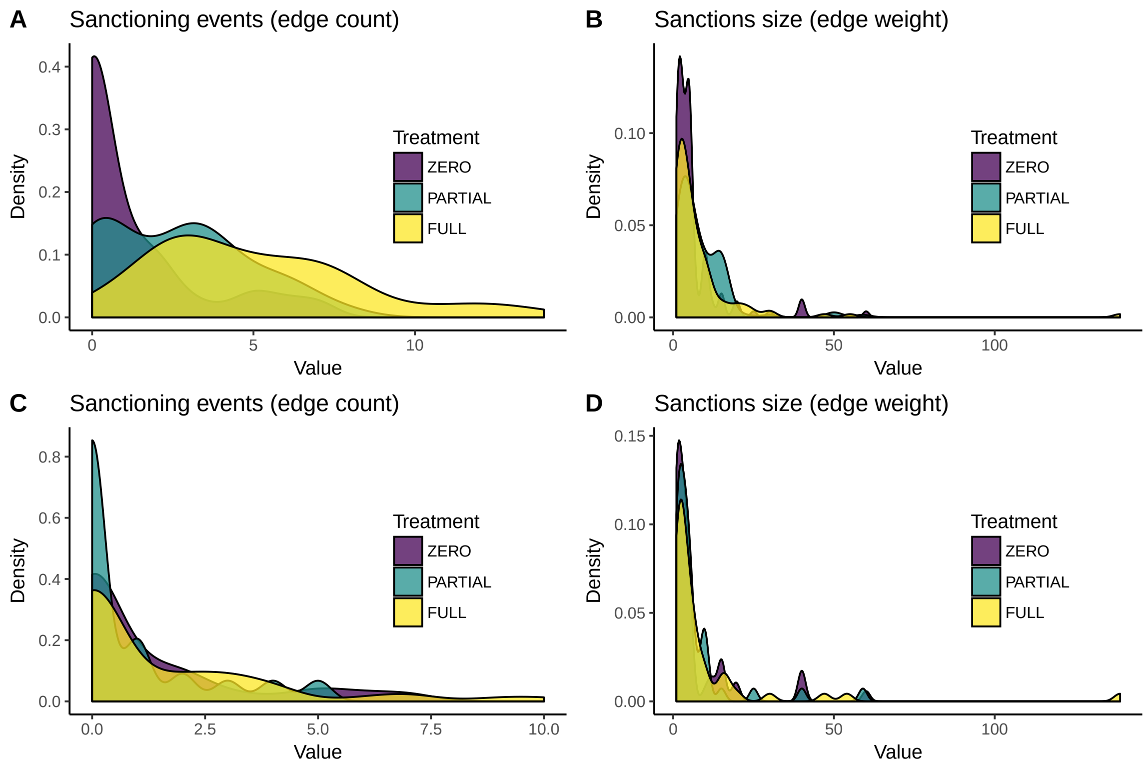


Figure 3.5: Summary plots of aggregate enforcement networks across time and treatments.

Across treatments there are similar distributions for the sizes of sanctions and the number of unique punishment recipients by a given subject. While sanction sizes were consistent across treatments, the number of sanctioning events (graph density, the edge count of an enforcement network) increases from Zero Monitoring to Full Monitoring. This is to be expected since opportunities to punish increase in that order (one additional opportunity in Partial Monitoring and three additional opportunities in Full Monitoring). Moreover, in Panel C there is an increase in sanctioning events is due mostly to the addition of sanctioning opportunities on outsiders. In other words, the densities of the insider enforcement networks are consistent across treatments.

Table 3.1 summarizes the enforcement networks on three dimensions: the number of "active nodes" (i.e. punishers), the edge count (number of sanctioning events) and edge weight (sanction size). Summaries are provided for in-group and out-group enforcement with standard deviations in parentheses. The tables capture interesting changes in enforcement structure. Perhaps most interesting is the non-monotonicity seen in insider enforcement. The number of punishers and the number of sanctioning events decrease from Zero Monitoring to Partial Monitoring and then increases in Full Monitoring. However, sanction size increases in Partial Monitoring but decreases in Full Monitoring. Turning to outsider deterrence, what is most surprising about Table 3.1 is that there were fewer punishers on average in Partial Monitoring than Full Monitoring.

Table 3.1: Summary statistics of enforcement networks.

(a) Insider regulation

	Zero Monitoring	Partial Monitoring	Full Monitoring
Number of punishers	0.817	0.683	1.000
	(1.14)	(1.07)	(1.40)
Sanctioning events	1.217	0.883	1.483
	(1.96)	(1.47)	(2.36)
Sanction size	6.73	10.18	3.63
	(13.97)	(28.78)	(6.98)

(b) Outsider deterrence

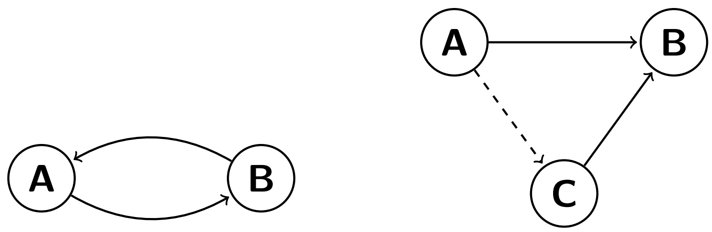
	Zero Monitoring	Partial Monitoring	Full Monitoring
Number of punishers	0.00	1.75	2.40
	(0.00)	(1.58)	(1.78)
Sanctioning events	0.00	1.75	3.53
	(0.00)	(1.58)	(2.55)
Sanction size	0.00	9.63	14.68
	(0.00)	(13.3)	(17.2)

3.3.2 Clustering

I am also interested in the clustering of enforcement networks. In fixed groups with repetition it is possible that certain subjects are consistent punishers while others consistently receive punishment, perhaps independent of harvest or poaching decisions.

High clustering by a subset of subjects in each group would indicate that the dynamics and structure of the network are driven by just a few individuals.

Two simple measures of clustering in networks are reciprocity and transitivity. (Note that I use the network science definition of these terms they do not refer to strategies or preference relations as they are commonly used in economics and game theory.) Figure 3.6 illustrates.



(a) Reciprocity: A and B sanction each other in period t

(b) Transitivity: A sanctions B, C sanctions B, and A "closes the triangle" by sanctioning C, all in period t.

Figure 3.6: Network clustering

Reciprocity captures clustering at the dyadic level, showing instances where two individuals assign sanctions to each other. For example, Panel A shows two subjects, A and B, exchanging sanctions in period t. Note that reciprocity does not capture inter-temporal exchanges, such as the case where A punishes B in t and B responds by punishing A in $t+1$. Since insiders were the only ones who could assign sanctions, reciprocity captures clustering at the in-group level. Reciprocity is calculated as the total number of reciprocal dyads – e.g. A punishes B and B punishes A – divided by the total number of edges, so the measure falls between zero and one.

Transitivity, also referred to as the clustering coefficient, captures global clustering in a graph by summarizing the rate at which connected triples close to form triangles. In Panel B, A is punishing C and C is punishing B. When calculating transitivity, I are looking for the number of times A "closes the triangle" by punishing C or vice versa. For example, if Subject A punishes Subject B, and subject B punishes subject C, I are interested in whether Subject C punishes Subject A or vice versa.[8] Connected networks exhibit high transitivity while disconnected networks exhibit low transitivity. Therefore, if an enforcement network displays high transitivity, it means the network is densely connected and that many subjects were either on the giving or receiving end of sanctions in a certain period. Transitivity is calculated as the total number of closed triangles divided by the total number of triples and falls between zero and one.

The aggregate clustering distributions are shown in Figure 3.7. Panels A and B show reciprocity and transitivity for aggregate enforcement networks. Panels C and D show reciprocity and transitivity for insider enforcement networks. Interestingly, higher levels of reciprocity in Zero Monitoring are seen, where insiders have the fewest sanctioning targets, compared to Partial and Full Monitoring. I also see some higher levels of transitivity in Zero Monitoring compared to the other treatments. This suggests that there is more clustering in the enforcement network when there are fewer punishment opportunities. Moreover, when I restrict our focus to the insider enforcement networks, there are higher levels of reciprocity and transitivity in Full Monitoring.

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[8]The network in Figure 3.6 Panel B, in which three nodes are connected by two edges, is often called a "connected triple" or a "2-star network".

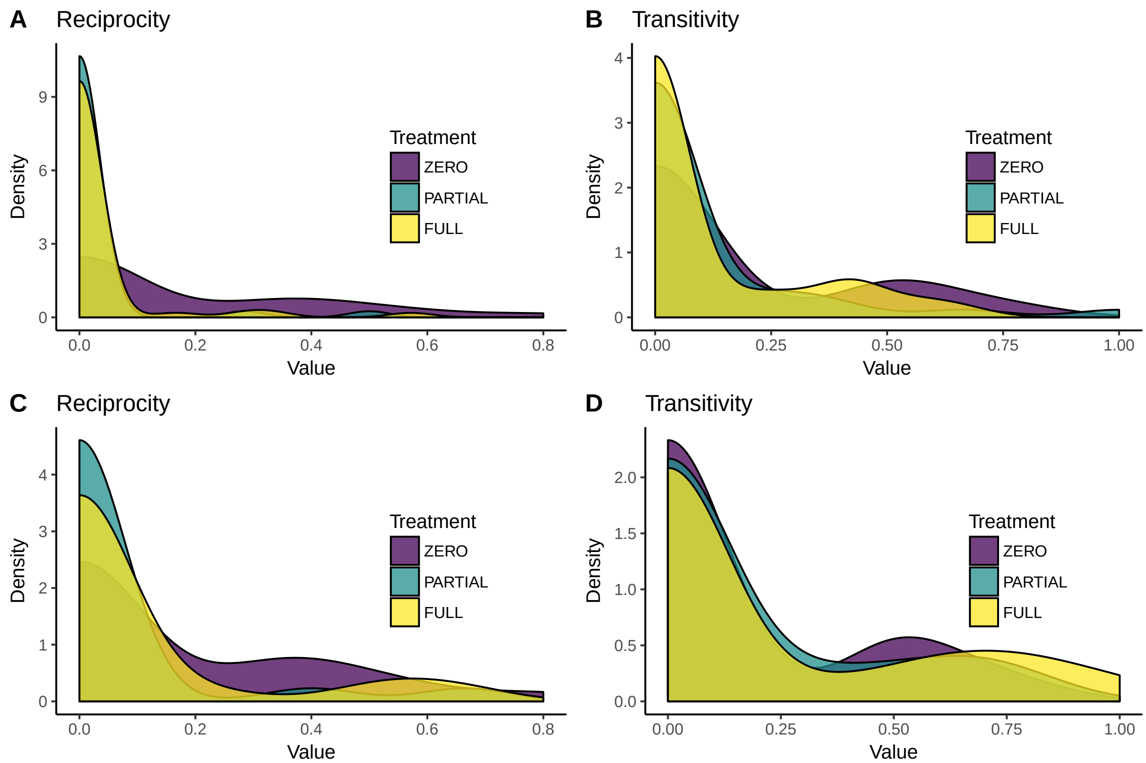

Figure 3.7: Clustering distributions. Panels A and B show reciprocity and transitivity for aggregate enforcement networks. Panels C and D show reciprocity and transitivity for insider enforcement networks.

Finally, I examine the structure of enforcement network through graph partitioning by using unsupervised methods to find vertices in a network that tend to cluster together. In the networks literature this is more commonly known as community detection (Jackson et al., 2008). One common method for detecting communities, or densely connected components in a graph, is to count the number of eigenvalues of the Laplacian of a graph that are greater than zero (Kolaczyk and Csárdi, 2014).[9] The number of positive eigenvalues corresponds to the number of subgraphs in the graph. I can interpret this as a measure of network structure. If sanctions are directed by a

[9]The graph Laplacian is given by $\mathbf{L} = \mathbf{A} - \mathbf{D}$ where $\mathbf{A}$ is the adjacency matrix and $\mathbf{D}$ is a diagonal matrix of the degree sequence of the graph, the list of connected, directional pairs (i.e. the degree matrix).

subset of individuals to another subset of individuals, then we should expect to see more than one partitioning of the enforcement network.

I carried out this calculation for the aggregate enforcement network for each group in each treatment. Our results are shown in Figure 3.8 Panel E. The axes display the graph index–the number of possible partitions, which is simply the number of vertices or subjects–and the eigenvalues of the graph Laplacian. Across treatments and groups I find multiple subgraphs. This corroborates other evidence of clustering or a tendency for sanctions to flow between a subset of subjects in a group.[10]

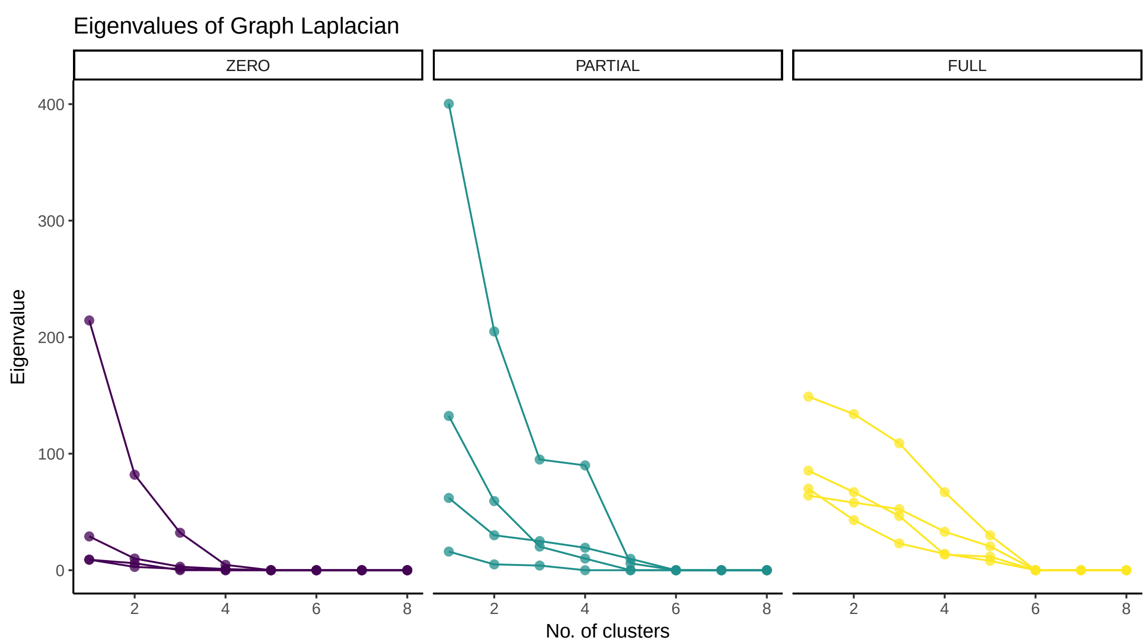

Figure 3.8: Community detection in aggregate enforcement networks using the Graph Laplacian.

3.3.3 Simulation

I have shown that summary visuals of enforcement networks can reveal their latent structures. I observe the emergence of leading punishers and clustering around

[10]Note that the eigenvalues for indices six through eight are zero in each group. This is because the graphs are directed and constrained by the fact that outsiders could not assign sanctions.

them across treatments. The number of sanctioning events goes up with sanctioning opportunities, and so too the number of unique subjects sanctioned by a subject in a given period, but the size of sanctions does not. Despite such similarities, interesting differences emerge as well. Insiders punish each other more when there are fewer opportunities to punish outsiders.

However, it is necessary to determine whether the structures observed in these enforcement networks were not randomly assembled. While I expect that sanctioning behavior is strategic and thus non-random, it is possible that insiders sanctioned for no discernible reason. If so, then the structure, namely the degree of reciprocity and transitivity, in the network that emerges from random sanctions would be similar to that of the observed networks. I can test whether this is the case using simulation.

I test the significance of the structure in the observed networks by simulating random, directed networks. A common procedure for simulating random graphs is given by the Erdős-Rényi network-generating algorithm (Erdős and Rényi, 1960). For a finite set of vertices, edges are formed as follows. Each vertex goes to each possible target and flips a coin. If the outcome is heads (or tails), the vertex forms an edge to the target. If not, no edge is formed. Once the vertex has flipped a coin for each target, the algorithm moves on to the next vertex, and so on. Reciprocity and transitivity can then be calculated for the whole network. Simulating a large number of random networks will lead to a Gaussian distribution of these structural measures. These measures range between zero and one, so the expected value for reciprocity and transitivity in a completely random network is 0.5.

However, this model discards important information from our setting. Namely, some agents are constrained from sending sanctions (forming edges), and in some treatments, those same agents are be constrained from receiving sanctions. Accordingly, I wrote an adjustment of Erdős-Rényi model to simulate constrained, random, directed networks. The algorithm proceeds as follows. At the start of a simulation, some nodes were randomly chosen as "outsiders" (nodes that cannot form edges but can receive edges, depending on the treatment) and "insiders" (nodes that can form edges to insiders and outsiders, depending on the treatment, and receive edges from other insiders.) Once all constraints were set, the networks were generated according to the Erdős-Rényi model.

For each group in each treatment, I simulated 1000 random networks to obtain a distribution of reciprocity and transitivity. I then compared the means of these distributions to the observed measures for a given group. The idea is that if the observed measure falls more in the tails of the simulated distribution then it is probably not random.

I also simulated 1000 random networks that preserve the degree distribution of each node. The idea is to suppose that some agents are more likely to issue sanctions, but they don't care who receives them. This simulation simply shuffles an observed network while taking care to make sure that a given node has the same degree as in the observed network (i.e. same number of sanctions received and sent).

Our results are shown in Figures 3.9 - 3.11. Treatments are plotted separately. In each plot there are two panels for each group: one for reciprocity and the other for transitivity. Each group has a plot for reciprocity and transitivity. The vertical black line indicates the observed reciprocity or transitivity for a group's aggregate enforcement network. The simulated networks that do not preserve degree distribution are plotted in green. The simulated networks that do preserve degree distribution are plotted in blue. The means are shown with vertical lines. In general, a significant effect will be illustrated by a black line (the observed value) in the tails of the simulated, random distribution (the shaded green area).

By and large, the results suggest that the enforcement networks I observe are significantly different from random. For groups that had reciprocity or transitivity scores of zero it is evident that they are different from random. It is more interesting to look at groups where transitivity and reciprocity were positive.

First consider reciprocity. In Zero Monitoring, Groups 1 and 4 had positive levels of reciprocity that lie between the right tail and the mean of the random distribution, suggesting weak significance. In Partial Monitoring, Groups 1, 2 and 3 show significant reciprocity. The same holds for Groups 1 and 3 in Full Monitoring. Moving on to transitivity, in most cases the observed value aligns more closely with the mean of the simulated networks that preserve degree distribution, with a few exceptions such as Group 1 in Zero Monitoring and Group 1 in Partial Monitoring. These results suggest that the order we see in enforcement networks is unlikely to have arisen purely by chance.

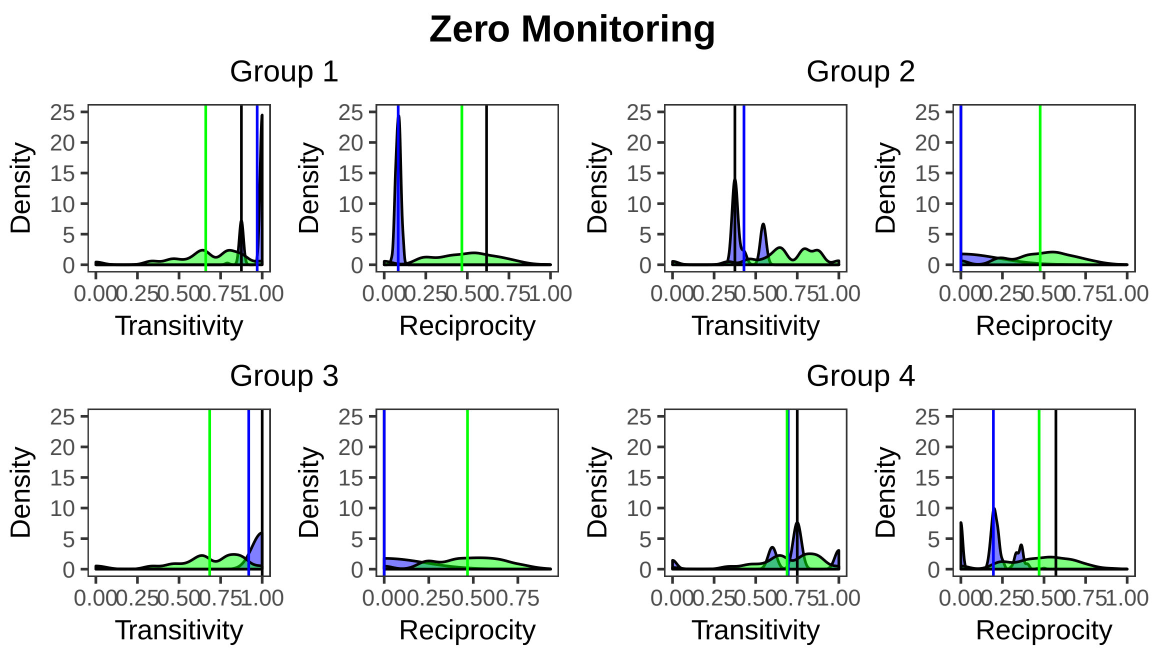

Figure 3.9: Simulated enforcement networks: Zero Monitoring

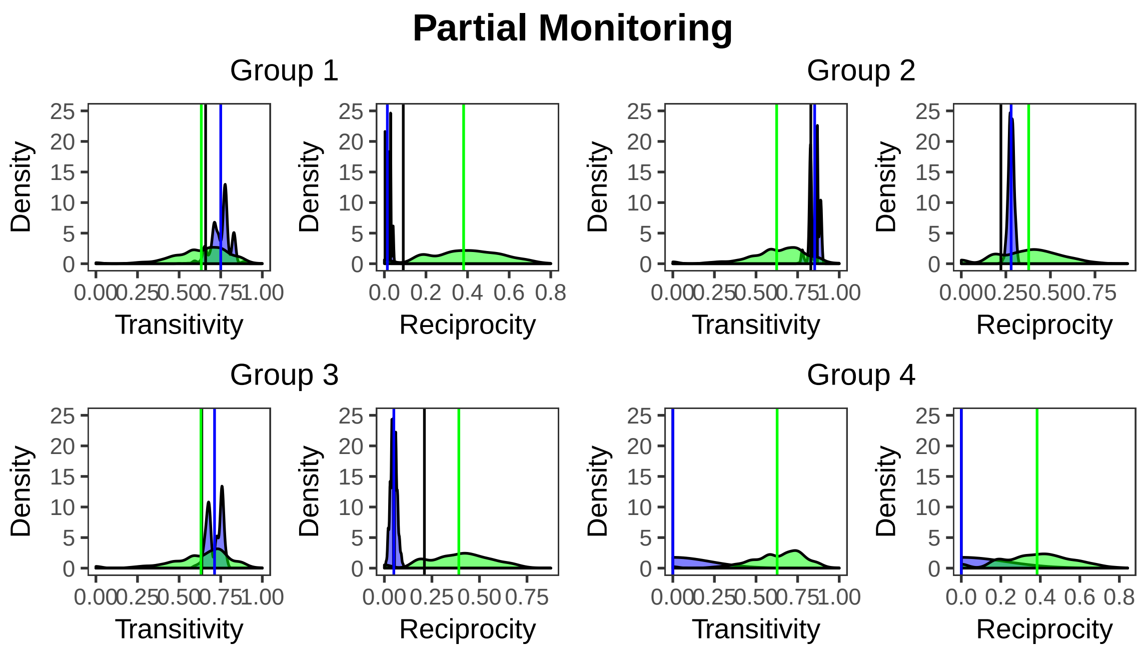

Figure 3.10: Simulated enforcement networks: Partial Monitoring

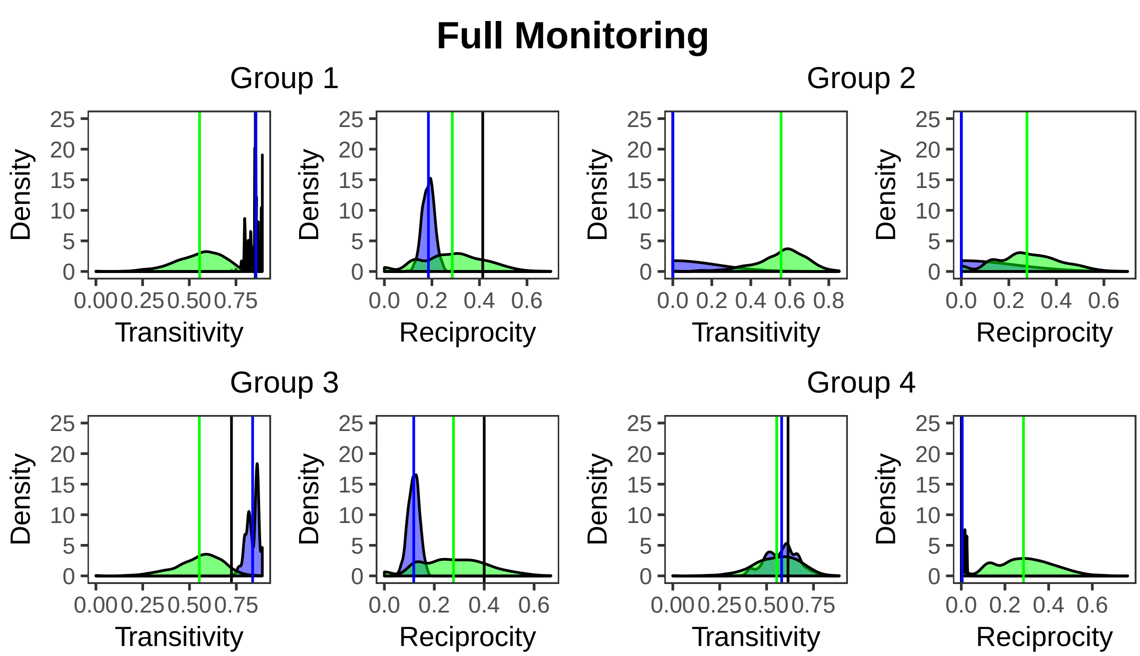

Figure 3.11: Simulated enforcement networks: Full Monitoring

3.4 Modeling enforcement

3.4.1 Predicting sanctioning behavior

A key question in decentralized enforcement is what drives sanctions between subjects. This is a challenge because behavior within a group depends on subject characteristics, such as type, appropriation decisions and payoffs, and group characteristics, such as aggregate appropriation and enforcement network structure. Moreover, a subject simultaneously chooses sanctions for multiple subjects, and given that sanctions are usually subject to a binding constraint, like initial payoffs or experiment design, it is unlikely that her sanctioning decisions for each target are independent.[11]

One way to think about modeling sanctioning behavior is to separate the intensive margin from the extensive margin: the decision to punish from the decision how much to punish. In other words, I could model edge formation separately from edge weight, assuming that a subject is trying to optimally allocate sanctions subject to her budget constraint and her beliefs about what other subjects will do. The networks literature has developed a variety of statistical methods for modeling networks. The canonical model is the Exponential Random Graph (ERGM), an analogue to generalized least squares that predicts edge formation as a binary random variable using endogenous (e.g. subject attributes) and exogenous (e.g. network structure) information (Kolaczyk and Csárdi, 2014). More recently, Generalized Exponential Random Graphs (GERGMs) have been developed to extend the ERGM framework to model edge formation and edge value in directed graphs (Wilson et al., 2017). However, because these models rely on simulation to obtain parameter estimates, either via Markov Chain Monte Carlo, Gibbs Sampling or Metropolis-Hastings, they are sensitive to

[11] De Geest et al. (2017) impose both endogenous and exogenous constraints on sanctions: an subject can only sanction up to her initial payoffs, but she cannot reduce her target's payoffs below zero. Other studies impose only exogenous constraints. For example, Reuben and Riedl (2013) and Fehr and Gächter (2002) and many others impose an upper bound on punishment to prevent high-earners from punishing more than low-earners. However, Reuben and Riedl (2013) find that the upper bound did not strongly influence behavior: only 0.85% of all sanctions touched the limit.

degeneracy. This occurs when the simulated probability density function places too much density on a small number of observations, often resulting in the model failing to converge. Enforcement networks in social dilemma networks tend to be sparse, on aggregate but especially from period to period, so it is likely that these methods will be biased towards predicting zero edge formation and edge weight. Finally, both ERGMs and GERGMs presume that edges are independent events. As noted above, this assumption is likely to fail when modeling enforcement.

I instead take a two-stage approach to predict sanctioning behavior that exploits the network structure of our data. I first estimate the likelihood a subject sanctions another subject. Then, given the formation of an edge, I estimate the expected value of sanctions. Hurdle models are often used to model processes that generate count data, in which the minimum value of the dependent variable is zero and positive values are integers. I model the intensive margin with a probit model and then model the extensive margin using a negative binomial regression to account for over-dispersion in sanctions. I estimate separate models for each treatment to account for treatment-specific constraints on the evolution of enforcement networks. Our full specification is

$$\begin{aligned} Pr(s^j_{kit} > 0) &= \Phi(\alpha + \beta \mathbf{P'}_{i,j,kt} + \theta \mathbf{G'}_{kt} + \gamma \mathbf{X'}_{ki,t-1} + \eta_k + \nu_t) \\ \mathbb{E}[s^j_{kit} | s^j_{kit} > 0] &= \exp(\alpha + \beta \mathbf{P'}_{i,j,kt} + \theta \mathbf{G'}_{kt} + \gamma \mathbf{X'}_{ki,t-1} + \eta_k + \nu_t) \end{aligned} \tag{3.1}$$

where s^j_{kit} is a weighted sanction from i to j in group k and period t; $\mathbf{P'}_{i,j,kt}$ include controls for the initial payoffs in t to j and i respectively, as well as a dummy variable, "Outsider", indicating whether j was an outsider; and $\mathbf{G'}_{kt}$ include controls for aggregate insider harvest and outsider poaching in group k and period t. The next two terms are network controls. $\mathbf{X'}_{ki,t-1}$ are individual enforcement network measures in $t-1$: number of sanction events received and their corresponding value, and the number of sanction events assigned and their corresponding value. These can be thought of as subject i's in-degree and out-degree broken down into edges and weights. Finally, η_k and ν_t are group and period fixed effects, respectively. Standard errors are clustered at the group level to control for any remaining within-group correlation.[12]

Our focus here is on the partial effects of the individual network measures. At the individual level I are interested in how a subject's "position" in the previous period's enforcement network affects her use of sanctions. Position is simply a function of sanctions received and sanctions sent. The contribution here is to distinguish effects from edges and their values. It is possible that the number of sanctioning events and the size of sanctions have different effects on behavior. This is because a sanctioning event can send multiple signals. For instance, in De Geest et al. (2017) sanctions are intended to supply two desired behaviors: cooperation by insiders ("harvest the socially optimally level") and non-encroachment by outsiders ("do not harvest the resource at all").

Our results are reported in Table 3.2. Models (1), (3) and (5) estimate the intensive margin $Pr(s^j_{kit} > 0)$ while models (2), (4) and (6) estimate the extensive margin $s^j_{kit}|s^j_{kit} > 0$.

[12]Note that sanctions used on the right-hand side are not the same as the left-hand side. Sanctions on the right-hand are sanctions supplied by subject i to subjects $-i$ in t. Sanctions on the left-hand side are sanctions received by subject i from subjects $-i$ in $t-1$.

Table 3.2: Estimating sanctions and sanction size (edge formation and edge weight).

	Zero Monitoring		Partial Monitoring		Full Montioring	
	(1)	(2)	(3)	(4)	(5)	(6)
Target initial payoff	0.052***	0.021**	0.033***	0.012**	0.041***	0.016***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)
Out-degree (weight)	0.023***	0.064***	-0.002	0.055***	0.012	0.081***
	(0.00)	(0.01)	(0.00)	(0.02)	(0.02)	(0.01)
Out-degree (edges)	0.423***	-0.482***	0.322***	-0.319***	0.225***	-0.157*
	(0.10)	(0.10)	(0.10)	(0.10)	(0.04)	(0.08)
In-degree (weight)	0.056***	0.005	-0.014***	0.008*	-0.073***	-0.033
	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.04)
In-degree (edges)	-0.158**	-0.677***	0.268***	0.409***	0.247***	0.192
	(0.08)	(0.20)	(0.09)	(0.11)	(0.08)	(0.13)
Own initial payoff	0.009**	0.014	-0.016***	-0.010**	-0.002	-0.002
	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)
Total insider harvest	0.100**	0.063	0.080**	0.018**	0.082***	0.067***
	(0.04)	(0.08)	(0.04)	(0.01)	(0.02)	(0.01)
Total outsider poaching	0.231***	0.168	0.028	-0.097**	0.148***	0.056**
	(0.06)	(0.11)	(0.02)	(0.04)	(0.02)	(0.03)
Outsider			1.234***	-0.185	0.721***	0.055
			(0.30)	(0.17)	(0.27)	(0.06)
Constant	-19.313***	-8.928	-7.616**	2.059	-13.152***	-3.966***
	(4.28)	(7.47)	(3.10)	(1.71)	(1.34)	(1.50)
Group fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Period fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
N	1040	63	1400	154	1960	280

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The coefficient to Target Initial Payoff is highly significant across treatments. This is intuitive: all else equal, subjects with higher payoffs tended to receive more sanctions. Interestingly, I do not find a consistently significant association between a subject's own payoff and her sanctioning behavior. In Partial Monitoring the association is negative and significant, indicating that her use of sanctions was inversely related to her payoffs. The sign for both coefficients to Own Initial Payoff are also negative in Full Monitoring but the effect is insignificant. Finally, the effect of Type is positive and significant on the intensive margin only. Outsiders were more likely to get hit with sanctions, but they were not more likely to get sanctioned harder.

I now turn to the individual network measures. Across treatments I find a consistent effect of out-degree edges (the numbers of sanction events received). On the intensive margin the effect is positive, suggesting that when subjects received more unique sanctioning events in the previous period, they were more likely to use sanctions themselves in the next period. However, the effect on the extensive margin is negative, suggesting that they assigned smaller sanctions in response to sanctioning events. Interestingly, the effect is flipped for out-degree weight. When subjects received larger sanctions, they were more likely to assign larger sanctions. Two points can be drawn from these findings. First, subjects choose sanctions in part to respond to sanctions they received. This is consistent with other studies that find instances of retaliatory punishment (e.g. Nikiforakis (2008)). Second, subjects react differently to the sanction events and sanction size.

Subjects also refer to their own history when choosing sanctions. On the intensive margin, I find that the effect of lagged in-degree edges is positive, although the coefficient in Zero Monitoring is insignificant. However, in Partial and Full Monitoring, the effect is negative and significant. Once more the exception is Zero Monitoring, where the coefficient is positive and significant. Notice that the magnitude of the positive effect is higher in Full Monitoring. As monitoring of outsiders increases, the sign of the effect flips from negative to positive and then increases in magnitude.

3.4.2 Evaluating network efficiency

I conclude this section with a simple empirical model that describes the marginal benefits of enforcement on changes in group payoffs.

Groups have to balance the benefits of cooperation with the costs of enforcing it. Insiders in De Geest et al. (2017) used sanctions to restrict appropriation of the resource and the authors estimated the efficacy of sanctions by estimating changes in individual harvest or poaching decisions over time and controlling for deviations from the group average and sanctions received in the previous period. Their findings suggest that sanctions had little effect on individual behavior. However, I have shown here that from one period to the next enforcement networks can change in three ways. More or fewer insiders can punish; the same punishers can form new edges in the network (i.e. punish a different set of subjects); or the same punishers can levy different sanction sizes on the same set of subjects.

To better understand the relationship between sanctions and behavior, I estimate how changes in the structure of enforcement networks along the three dimensions above associate with changes in group payoffs. The advantage of this approach is that it exploits the full network structure of the data. The disadvantage is that for outsider enforcement I have to estimate separate models for each treatment due to constraints imposed by the experiment design. In Partial Monitoring insiders could only monitor and sanction one outsider in each period. The marginal effect of punishing an additional outsider on aggregate outsider harvest therefore cannot be identified. Moreover, there is a concern with some of our independent variables due to censoring.[13] The number of sanctioning events and the number of punishers are constrained differently across treatments by the experiment design. In all enforcement networks the number of punishers is bounded by $[0, 5]$.[14] In the insider enforcement networks, the number of sanctioning events is bounded by $[0, 20]$. In the outsider enforcement networks it is bounded by $[0, 15]$. In order for the estimated coefficients for these variables to be interpreted as the marginal effect on the change in aggregate insider harvest or outsider poaching from an additional punisher or sanctioning event,

[13] See Rigobon and Stoker (2007) for a discussion of regression with censored independent variables. The authors point out that censorship can lead to biased coefficients if observations pile up at the boundary points. However, if the independent variables are exogenously censored, meaning that censorship is not connected with the dependent variable, then unbiased estimates can be obtained through complete case analysis in which boundary observations are dropped.

[14] For any network with n vertices and no cycles (edges between a vertex and itself) the number of possible edges is $m = n(n-1)$.

I removed any observations that met the upper bound. Fortunately, as shown in Figure 3.12, there were few.

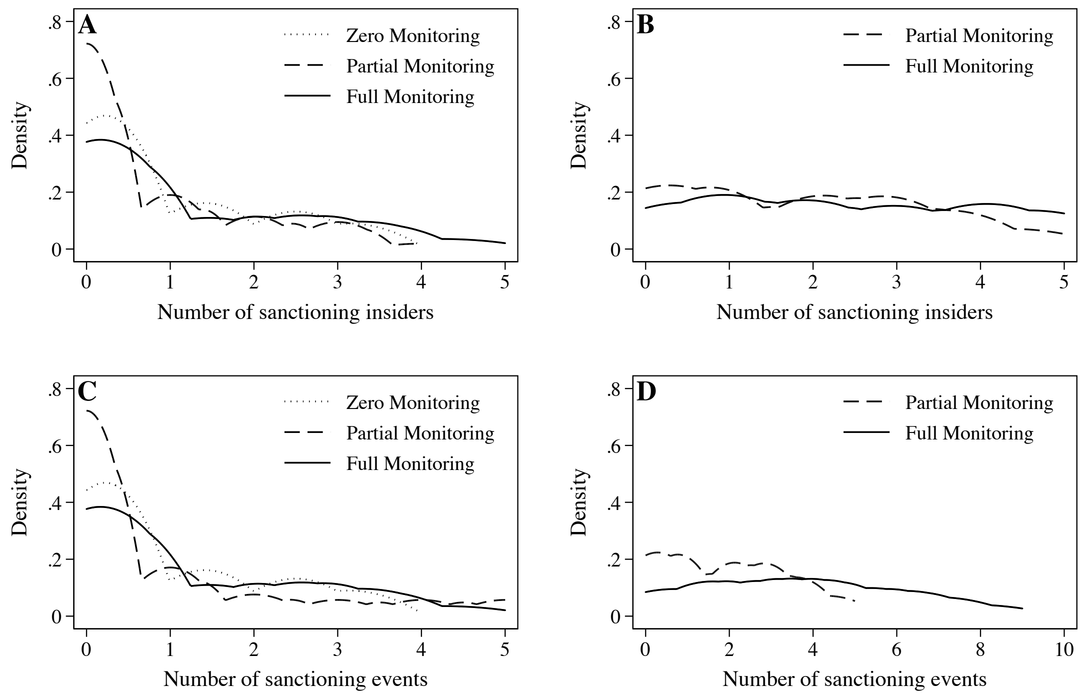

Figure 3.12: Densities of punishers and sanctioning events across treatments. Panels A and C show the densities for insider enforcement networks. Panels B and D show densities for outsider enforcement.

Given our constraints and the relatively small sample size, these estimations are speculative. Nevertheless, they are a useful starting point for examining the marginal benefits of different ways to allocate sanctioning responsibilities. Our full specification is the probit model

$$\begin{aligned}\Delta\pi_k^{IN} = \Phi(&\alpha + \beta_1 \text{Punishers}_{kt}^{IN} + \beta_2 \text{Sanction events}_{k,t-1}^{IN} + \beta_3 \text{Total sanctions}_{kt}^{IN} \\ &+ \gamma_1 \text{Punishers}_{kt}^{OUT} + \gamma_2 \text{Sanction events}_{kt}^{OUT} + \gamma_3 \text{Total sanctions}_{kt}^{OUT} \\ &+ G_{kt}^{IN} + G_{tk}^{OUT} + \nu_k + \epsilon_{kt})\end{aligned} \tag{3.2}$$

where $\Delta\pi_k$ is a binary outcome equal to one if group k payoffs between periods t and $t-1$ increase, $\text{Punishers}^{IN}_{k,t-1}$ are the number of insiders punishing other insiders in $t-1$, $\text{Sanction events}^{IN}_{k,t-1}$ are the number of sanctioning events (edge count), and $\text{Total sanctions}^{IN}_{k,t-1}$ is the total number of sanctions (summed across punishers). The same definitions apply for variables with the "OUT" superscript (i.e. $\text{Punishers}^{OUT}_{kt}$ is the number of insiders punishing outsiders). Finally, G^{IN}_k and G^{OUT}_k are aggregate insider harvest and outsider poaching, and ν_k is a vector of group fixed effects. Standard errors are clustered at the group level to control for any remaining serial correlation within groups.

The intuition behind this model is that different configurations of an enforcement network have different marginal benefits. Adjusting the number of punishers, the number of sanctioning events and the volume of sanctions may have different effects on the credibility of punishment as an institution to corral unwanted behavior. This in turn will affect how punishment improves group welfare, if at all.

Our results are reported in Table 3.3. The main result is the varying affect of the number of punishers on in-group enforcement. In Zero Monitoring the effect is negative and significant, indicating that adding additional punishers to enforce in-group behavior reduced group welfare. However, the effect is positive and significant in Partial and Full Monitoring – adding punishers to regulate insiders improved group welfare. The variable Punishment Events captures the marginal benefit of adding a binary sanctioning event, either to enforce in-group or out-group behavior, holding constant the number of punishers and the current volume of punishment. The effect is negative across treatments and significant in Partial Monitoring. Interestingly, I find no significant effects for outsider enforcement. This corroborates De Geest et al. (2017) who find that outsiders generally did not respond to punishment.

Table 3.3: Estimating the efficacy of insider enforcement.

	(1) Zero Monitoring	(2) Partial Monitoring	(3) Full Monitoring
Punishers (in-group enforcement)	-0.335***	0.261**	0.199**
	(0.02)	(0.13)	(0.08)
Punishment events (in-group enforcement)	-0.087	-0.438***	-0.053
	(0.16)	(0.16)	(0.21)
Punishment volume (in-group enforcement)	-0.011	-0.032***	-0.034
	(0.01)	(0.01)	(0.04)
Insider harvest	-0.048	-0.135*	-0.126***
	(0.05)	(0.07)	(0.05)
Outsider harvest	-0.209***	-0.173*	-0.350**
	(0.07)	(0.10)	(0.14)
Punishers (out-group enforcement)		0.088	-0.042
		(0.16)	(0.12)
Punishment events (out-group enforcement)		0.109	0.172
		(0.28)	(0.18)
Punishment volume (out-group enforcement)		-0.014	-0.013
		(0.01)	(0.03)
Constant	6.975***	6.483***	8.353***
	(0.96)	(1.96)	(2.10)
Group fixed effects	Yes	Yes	Yes
N	56	56	56

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.5 Enforcement dynamics

Enforcement networks can be thought to evolve according to a stochastic process that follows the Markov property. Enforcement can take on various levels of collective action and thus represents a finite set of states. In a given period, the network can be in one of these states, depending only on the previous state. So long as the Markov process is ergodic it has a unique stationary distribution describing the fraction of time spent in each state. I can use this fact to estimate the most likely structure of an enforcement network at any given time.[15] This means that for each group, I can estimate the long-run probability that enforcement takes on a particular structure.[16]

Our previous analysis suggests that one of the more important characteristics of enforcement networks is the number of sanctioning events rather than the volume of sanctions. It possible that the dynamics of the system can be captured by how insiders allocate sanctions between in-group regulation and out-group deterrence. Accordingly, I define the following four states:

Table 3.4: State space of the simulated enforcement networks.

State	**Description**
$x_1 : s_{in} = 0, s_{out} = 0$	No insider regulation, no outsider deterrence
$x_2 : s_{in} > 0, s_{out} = 0$	Insider regulation, no outsider deterrence
$x_3 : s_{in} = 0, s_{out} > 0$	No insider regulation, outsider deterrence
$x_4 : s_{in} > 0, s_{out} > 0$	Insider regulation, outsider deterrence

This creates a four-state Markov chain X_k where X_{kt} denotes the position of the chain in time t. The advantage of defining these states is that I exploit all available information in the networks in a parsimonious way. While it is possible to create more detailed states, this would not necessarily lead to a more informative simulation since the number of periods is fixed and thus increasing the state space will likely lead to more null transitions.[17] A disadvantage is that I cannot run the simulation for Zero

[15]A Markov chain that is aperiodic and irreducible is ergodic. Aperiodic means the chain does not cycle in a predictable way. Irreducibility means each state can be reached eventually by another state. See Häggström (2002) for a proof of the existence of a unique stationary state distribution in ergodic chains.

[16]Choi and Bowles (2007) use a similar approach. They statistically recover the Markov process governing an agent-based simulation to estimate the steady-state fraction of agent types in the population.

[17]I ran a similar simulation on the following three-state Markov chain X_k: $x_1 : n_{in} > n_{out}$; $x_2 : n_{in} < n_{out}$; $x_3 : n_{in} = n_{out}$, where n_{in} is the number of insiders sanctioning insiders and n_{out} is the number of insiders sanctioning outsiders. The results are available upon request. The

Monitoring. However, since I are interested in studying the trade-off between insider regulation and outsider deterrence, the data from Zero Monitoring cannot help us.

The transitions between states were taken from the experiment data as follows. I first created indicator variables for each state and in each period I mark which state the network was in. I counted the transitions from each state i to each state j and created a square matrix ($n \times n$, where n is the number of states) where element $\{i, j\}$ describes the number of instances the network moved from i to j across all fifteen periods of the experiment. I then created the transition matrix M_k for group k by normalizing the rows so they summed to one. Each element of M is then defined by $P(i, j)$, the one-period probability of moving from i to j.

For each treatment I then obtained a list of four ergodic transition matrices $\mathbb{M} = \{M_1, M_2, M_3, M_4\}$. Recall that ergodicity implies a unique stationary state distribution $\Psi^* = \Psi^* M^t \; \forall \; t$ that describes the long-run probability the chain is in a given state and is not sensitive to initial conditions, i.e. the starting state of the chain. This distribution can be estimated by simulating the marginal probabilities of X_k^t for sufficiently large t. The simulation begins by assigning the chain to a randomly-chosen state at $t = 0$ from distribution Ψ. For each subsequent t, the next state X_{t+1} is drawn from the distribution over the previous state, $X(S_t)$. If the simulation converges, then the average value of the time series for each state will approximate the stationary distribution. Not only can I can look at the stationary distribution, but also the time it took for the chain to converge it long-run behavior.[18]

disadvantage of this simulation is that it does not distinguish cases where $n_{in} = n_{out} = 0$ and $n_{in} = n_{out} > 0$.

[18]Another way to think about the steady-state is as an eigenvector of M. A steady state occurs when a probability vector multiplied by M returns the same probability vector.

I ran 1000 simulations for each group in each treatment. The results are reported in Table 3.5.[19] Each entry represents long-run probability a group was in a given state. Missing values indicate a group was never in a particular state.

Table 3.5: Estimated stationary states. Each entry represents long-run probability a group was in a given state.

	Partial Monitoring				**Full Monitoring**			
	Group 1	*Group 2*	*Group 3*	*Group 4*	*Group 1*	*Group 2*	*Group 3*	*Group 4*
x_1	0.18	NA	0.57	0.27	0.06	NA	NA	0.08
x_2	0.22	0.07	0.07	NA	0.20	0.21	0.07	0.08
x_3	0.23	0.59	0.07	0.58	0.52	0.71	0.36	0.64
x_4	0.36	0.34	0.29	0.15	0.22	0.07	0.57	0.20

Some important results emerge from this analysis. First, each group spends at least an expected fifty-percent of the time in one state, with the exception of Group 1 in Partial Monitoring. This suggests that enforcement within a group tended to follow a particular pattern. Second, two of the most visited states are $x_3 : s_{in} = 0, s_{out} > 0$ and $x_4 : s_{in} > 0, s_{out} > 0$). In particular in Full Monitoring, all groups spend a substantial amount of time in x_3. It seems that long-run network structure tends to move between states of outsider deterrence and joint enforcement (insider regulation and outsider deterrence) rather than purely between them, as observed by the relatively lower stationary probabilities for $x_2 : s_{in} = 0, s_{out} > 0$. Another difference between treatments is the stationary probability of x_1, the state of zero enforcement. Interestingly, groups in Partial Monitoring were more likely to be in this state than their counterparts in Full Monitoring.

Our analysis above describes the stationary structure of enforcement networks. I are also interested in the expected change in network structure from one period to the next. To this end, I calculate the net transition matrix $M^N = M - M^T$ where

[19]The time series of each simulation are shown in Figure C.1 and Figure C.2. Each figure plots the first 100 simulation trials and shows similar convergence times across groups and treatments.

element $p_{ij}^N = p_{ij} - p_{ji}$ represents the net transition from state i to j. Where $p_{ij}^N > 0$ the transition from i to j is more likely than the other way round.

Plotting a contour map of M^N provides a description of how each enforcement network is expected to evolve. A contour map of a net transition matrix is like a landscape where positive values indicate troughs and negative values indicate peaks. In troughs, the system will tend to remain put; on peaks, the system will tend to move. In Figures 3.13 and 3.14 lighter colors indicate where $p^N > 0$, increasing in brightness with larger positive values. These represent relatively stable spaces in the system, or troughs, while darker colors represent unstable spaces, or peaks. The goal of this visualization is to better understand stability in the enforcement networks across treatments.

Interestingly, more stable points emerge in Full Monitoring than in Partial Monitoring. This is consistent with the idea that the structure of enforcement changes more frequently in the limited monitoring treatment, suggesting that enforcement was less stable than in the complete monitoring treatment.

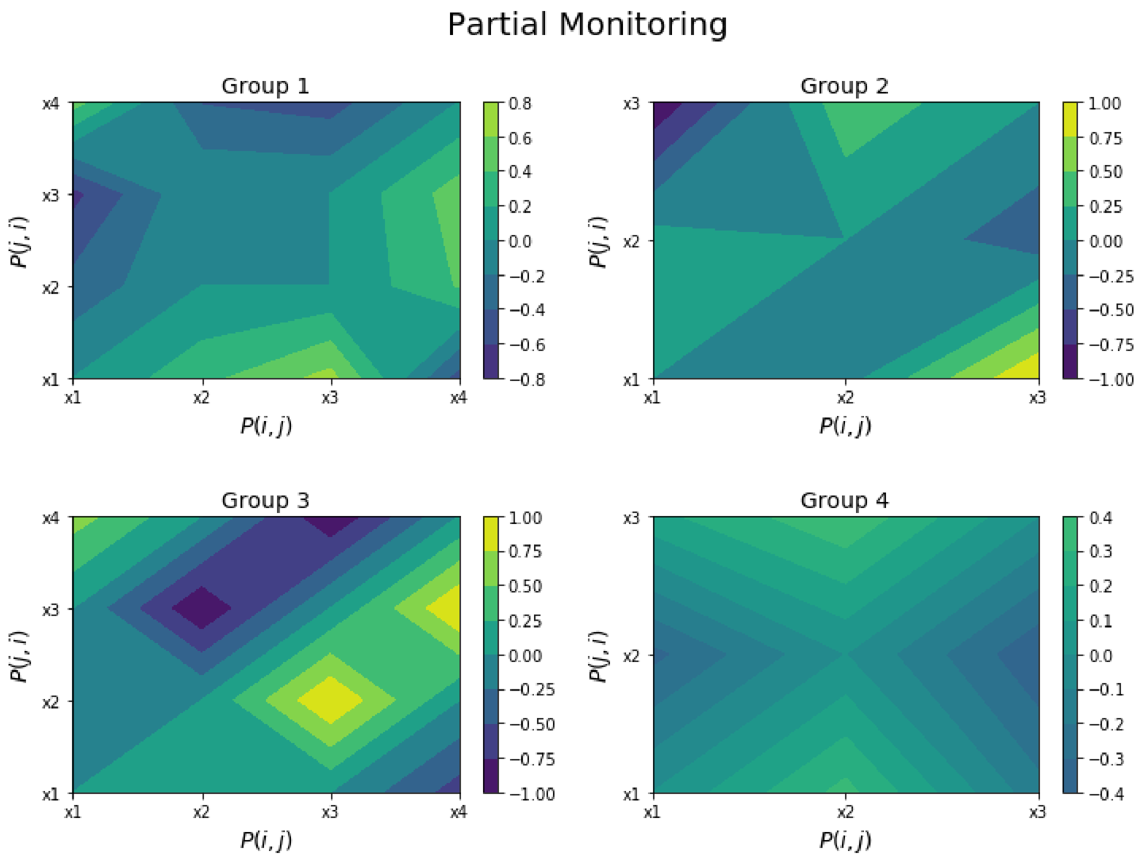

Figure 3.13: Contour plot of net Markov transition matrices in Partial Monitoring.

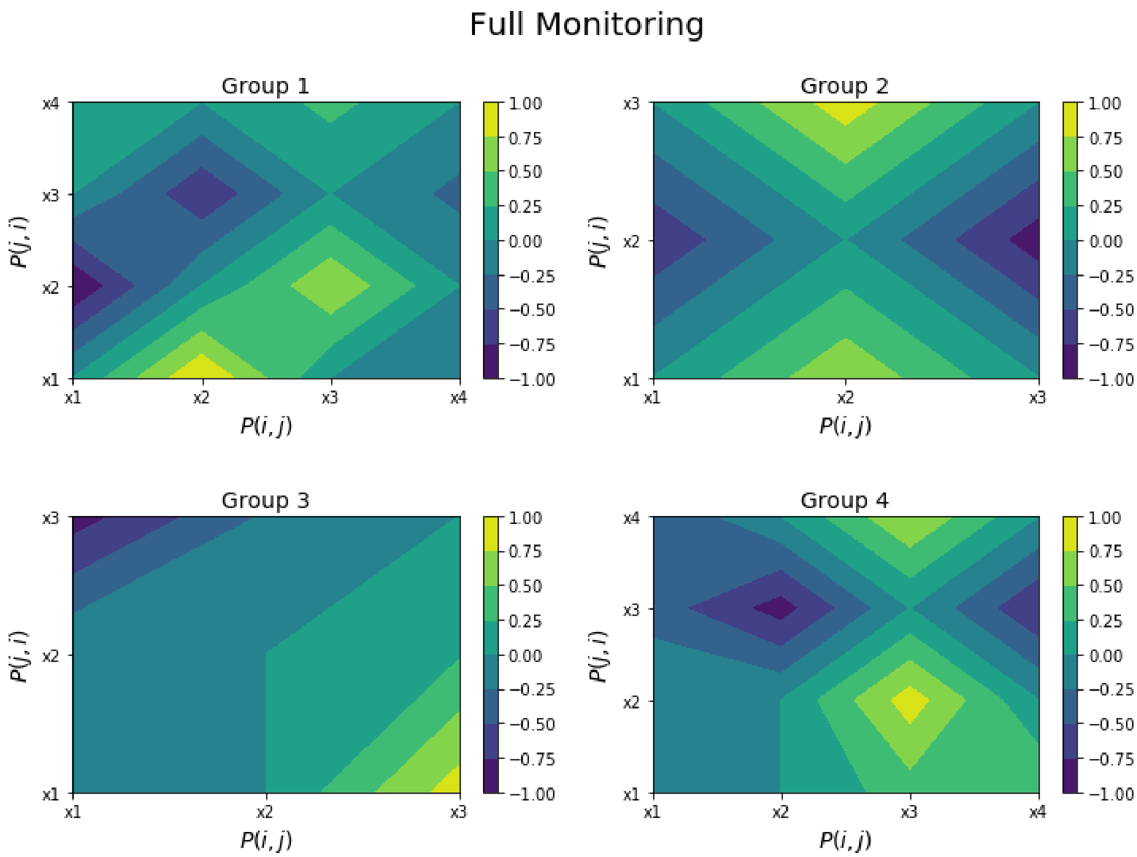

Figure 3.14: Contour plot of net Markov transition matrices in Full Monitoring.

Altogether, two conclusions can be drawn from these simulations. Stationary behavior suggests there is not a pure trade-off between insider regulation and outsider deterrence. This is consistent across groups and treatments. At the same time, there appears to be more stability in enforcement structure in the complete monitoring treatment, as indicated by the relatively higher frequency of stable points in the net transition matrices.

3.6 Conclusion

This chapter investigated the evolution of enforcement structure in a social dilemma experiment. Costly sanctions are capable of controlling deviant behavior when used correctly, but the coordination problem of allocation sanctioning responsibilities across individuals makes it possible for a variety of enforcement patterns to emerge. Taking a network approach to enforcement allows for a more detailed study of how enforcement is allocated, how it evolves, and ultimately, how effective it is at controlling behavior. Using data from a common-pool resource experiment with poaching by De Geest et al. (2017), where a group of insiders conserving a resource had to coordinate sanctions to regulate their own behavior as well as deter poachers, I find novel insights into decentralized enforcement.

I find that enforcement tends to cluster around a few individuals, although the structure of enforcement networks varies across treatments, with higher levels of reciprocity when insiders could not monitor poachers. I then estimate a hurdle model of the supply of enforcement to separately model the intensive margin and extensive margin of sanctions and find that insiders often supplied sanctions in response to sanctions they received. Moreover, in a model of enforcement efficiency, I find that adding an additional sanctioning insider to the network had a higher marginal benefit than simply adding sanctioning events or increasing the volume sanctions by insiders already engaging in punishment. Finally, a simulation of long-run enforcement network structure suggests that there is not a pure trade-off between regulating insiders and deterring outsiders.

There are limitations to our study. For starters, the experimental design of De Geest et al. (2017) imposed tight constraints on how enforcement networks could form and grow. Moreover, the relatively limited number of periods (15) of play made it unlikely that a credible threat could emerge from enforcement, given that previous studies found that it takes up to 50 periods for this to happen (Gächter et al., 2008). This also makes inference difficult since the data are zero-inflated. Future studies could exploit web platforms to run longer experiments with a variety of constraints on network formation to better understand how enforcement structure relates to enforcement efficacy.

These limitations notwithstanding, I have shown an alternative approach to analyzing enforcement data from social dilemma experiments. Since enforcement can be cast as a form of exchange and thus studied as a directed, weighted network that changes over time, our approach can be extended to other experiments that study exchange between subjects. Many economic experiments are programmed in z-Tree, which provides a straightforward method to collect exchange data through a feature called Contract Tables. The output from these tables is essentially an edge-list between subjects that describes who exchanged what and how much. This data can be converted into an adjacency matrix and then a graph object. This object can then be augmented with other experiment data to provide attributes to subjects, the vertices of the networks.

APPENDIX A

CHAPTER 1

Treatment regression results

	Insiders		Outsiders	
	(1)	(2)	(3)	(4)
	Aggregate	*First/Second*	*Aggregate*	*First/Second*
Partial Monitoring	0.003	-0.443	1.156	0.714
	(0.41)	(0.51)	(0.85)	(1.04)
Full Monitoring	-0.333	-1.014	-0.839	-0.440
	(0.41)	(0.53)	(0.76)	(1.00)
Second		-0.937**		0.637
		(0.30)		(0.46)
Partial Monitoring × Second		0.837*		0.827
		(0.37)		(0.61)
Full Monitoring × Second		1.277***		-0.747
		(0.38)		(0.53)
Constant	6.093***	6.593***	6.828***	6.488***
	(0.35)	(0.46)	(0.70)	(0.91)
R^2	0.007	0.031	0.072	0.095
N	900	900	540	540

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.1: Linear random effects regression models of treatment differences

Experiment instructions

The instructions below are for the Partial Monitoring treatment. We note where the instructions are changed for the Zero and Full Monitoring treatments.

Introduction

Welcome to today's session. You are here to participate in an experiment on decision making. The experiment will last approximately 60 minutes. Based on the decisions you make, and the decisions made by other participants, you will be paid in cash privately at the end of the session. Please turn off your cell phones and refrain from talking with other participants for the remainder of today's session.

We will now begin reading the instructions. A companion document, Screenshots, displays what you will see on your computer screen during each stage, written here in bold. Use these two documents together as we read through the instructions.

In addition, there is a Personal Record Table included with these instructions. You will fill in this table to keep track of your decisions and your Payoff during the experiment. During each period you should fill in this table with the information on your screen. Be sure to record your total payoff for each period in the rightmost column.

Sorting

At the start of the first period, and before the first stage, you and the other participants will be randomly sorted into groups of eight. These groups will then be randomly divided into two sub-groups: Group 1 (five members) and Group 2 (three members), as illustrated below. Each group of two subgroups is encased in an oval.

In this experiment you will interact within your oval, but you will not interact with other ovals.

When you receive your group assignment, you will also receive a group member identification number (ID). You will be in the same group and keep the same ID for the entire experiment.

At the start of the first period, and each subsequent period, you will receive an endowment of 12 tokens. You will make an investment decision with these tokens. We will describe this decision in detail momentarily.

During the sorting, you will see the screen **Sorting**. It will display your group number and your group ID.

You can click *Continue* to move to the **Waiting Screen**. The experiment will advance to the next stage when all participants have clicked *Continue.*

Stages

There are fifteen periods of decision-making in today's session. Each period is divided into five stages. The order, names, groups and duration of these five stages can be seen in the table below.

Number	**Name**	**Who's involved**	**Duration (seconds)**
1	Communication	Group 1	120
2	Investment	Group 1 & Group 2	30
3	Initial payoff	Group 1 & Group 2	20
4	Deductions	Group 1	60
5	Final payoff	Group 1 & Group 2	20

Conversion Rate

During the experiment your initial payoff and final payoff will be shown to you in laboratory dollars ($L). The conversion rate with US dollars is $1.00 L = $0.012 US. So if for example you see a payoff of $100 L, this converts to $1.20 US.

Questions

You are welcome to ask any questions you have about the experiment. If at any time you do have a question, or are unclear about something, raise your hand. Someone will come over to assist you.

If there are no questions at this time, we will now describe each stage.

1. Communication (Group 1)

For odd-numbered periods (i.e. 1,3,5 etc), this is the first stage. For even-numbered periods, this stage will be skipped.

Members of Group 2 will see **Waiting Screen** and will be asked to wait patiently until the next stage. Members of Group 1 can communicate with each other using a chat box and will see **Communication**.

As a member of Group 1, you can chat with the other members of Group 1. To send a message, type your text in the entry box, then press "Enter" on your keyboard to send. You will see your message displayed in the chat box with your group member ID next to it.

Your message will be visible on the computer screen of each member of Group 1. Please restrict your conversation to topics concerning the experiment.

Once the timer in the upper right corner reaches zero, the stage will end, and you will advance to the next stage.

If you finish chatting before the time is up, click *Continue*. When all of the Group 1 members have clicked *Continue*, or when the stage ends, the experiment will go on to the next stage.

2. Investment (Group 1 & Group 2)

This is the second stage. As a member of Group 1 or Group 2, you will see **Investment**.

In this stage, you will decide how much of your endowment to invest in The Account. What is left of your endowment will earn a private return of two-to-one. This means that one token not invested returns two dollars, two tokens not invested return four dollars, and so on.

What you earn from investing depends on two things: 1) how much *you* invest and 2) how much *everyone else* invests. The Initial Payoff Table at the back of these instructions shows you the relationship between how much is invested in The Account and total payoff from The Account. Note that The Account can yield negative returns.

How to make your decision

Your endowment is twelve tokens. This means that you can invest up to twelve tokens in The Account. You make your decision by entering a number into the entry box called "Your Investment in The Account". Your decision must be a whole number.

Payoff from your decision

Your decision determines what share of The Account Payoff you receive. You and the seven other participants in your oval will each get a share of The Account Payoff that depends on the tokens invested in The Account. Your share of The Account Payoff equals your number of tokens placed in The Account as a fraction of the total tokens you and the other participants in your oval place in The Account.

For example, if a total of 12 tokens are invested in The Account by the five members of Group 1 and the three members of Group 2, The Account Payoff is \$314.40L. If you placed 2 tokens in The Account then your share of The Account Payoff is $\frac{2}{12}$ of the \$314.40L, or $\frac{2}{12} \times \$314.40\text{L} = \$52.40\text{L}$.

Continuing with this example, your 10 tokens not invested would return you a private payoff of $10 \times \$2.00\text{L} = \$20.00\text{L}$. In addition, in each period you receive a fixed payoff of \$88.80L. Therefore, your initial payoff for this example is $\$52.40\text{L} + \$20.00\text{L} + \$88.80\text{L} = \$161.20\text{L}$.

Before making your decision, along with the Initial Payoff Table, use the payoff calculator to see what your payoff will be based on what you invest, what Group 1 invests, and what Group 2 invests. You may try as many combinations in the calculator as you like. They will not affect your payoff. Note that to use the calculator, you must first enter a number in the decision box. This acts as a placeholder while you use the calculator.

Please note that the lowest your initial payoff can be is zero.

When you have made your decision, click *Continue*. By clicking *Continue,* you will be asked to confirm your decision. You may revise your decision if you wish. When you confirm your decision, you will advance to the waiting screen. You cannot change your decision after this point. When all participants have confirmed their decisions, the experiment will go on to the next stage.

3. Initial Payoff (Group 1 & Group 2)

This is the third stage. As a member of Group 1 or Group 2, you will see **Initial Payoff**.

Here you can view your initial payoff. Your initial payoff is the sum of your payoff from The Account, your private payoff, and the fixed payoff. In addition, you will see: total investment in The Account; how much Group 1 invested; how much Group 2 invested; and the total payoff from The Account. Please make note of these amounts in your Personal Record Table.

Click *Continue* when you are ready to go on. After all participants have clicked *Continue,* the experiment will go on to the next stage.

4. Deductions (Group 1)

This is the fourth stage. Members of Group 2 will see **Waiting Screen** and will be asked to wait patiently until the next stage.

If you are a member of Group 1, you will see **Deductions**. You will be shown how much each Group 1 member invested, along with his or her initial payoff. You will see your information in the leftmost column, and you will see the information of the other Group 1 members in the first four columns to the right.

In addition, you will also see the information of one randomly chosen member of Group 2. You will see his or her information in the rightmost column. This Group 2 member will not know his or her information is being shown to you during this stage.[1]

The information of each participant you will see is his or her group number, group ID, investment, and initial payoff.

Note that this display format – you to the leftmost, the rest of Group 1 to the right, and one randomly chosen member of Group 2 to the rightmost – will be used in each period of today's session.

In this stage, you can decide to decrease the payoff of these participants by assigning deduction points. Only members of Group 1 can assign deduction points. Group 2 members cannot assign deduction points. If you are a member of Group 1, you may enter a number of deduction points for each participant. If you do not wish to change

[1]Note: This was changed for the Zero Monitoring treatment to read "You will *not* see any information of Group 2" and in the Full Monitoring treatment to read "In addition, you will also see the information of each member of Group 2. You will see their information in the rightmost columns."

the payoff of a participant, then you must enter 0.

Assigning Deductions

You will incur costs from assigning deduction points. Each deduction point you assign will cost you \$1L. For example, if you assign 2 deduction points to one participant, this costs you \$2L. If, in addition, you assign 4 deduction points to another participant, this costs you an additional \$4L. In total for this example, you will have assigned 6 deduction points, costing you \$6L. To view the cost of your assigned deductions, click the button *Cost*. Your deduction assignment cost is calculated as:

$$\text{cost of assigned deductions} = 1 \times \text{number of assigned deduction points}$$

You can change your decision as long as you have not left the stage. To recalculate the costs after changing your assigned points, simply click *Cost* again.

Please note, your cost of assigned deductions cannot exceed your initial payoff.

Receiving Deductions

If you assign 0 deduction points to a particular participant (i.e., enter "0"), you will not alter his or her payoff.

However, if you assign one deduction point to a participant, you will decrease his or her payoff by \$3L. If you assign a participant 2 deduction points you will decrease his or her payoff by \$6L, and so on.

Likewise, if you receive a total of one deduction point, your payoff will be decreased by \$3L. If you receive a total of 2 deduction points, your payoff will be decreased by \$6L, and so on. Your loss from received deductions are calculated as:

$$\text{loss from received deductions} = 3 \times \text{number of received deduction points}$$

Please note, your cost of received deductions cannot exceed your initial payoff.

Total Cost of Deductions

Putting all this together, if you are a member of Group 1, your total cost in this stage is:

$$\begin{aligned}\text{total cost of deductions} = (1 \times \text{assigned deductions}) +\\ (3 \times \text{received deductions})\end{aligned}$$

If you are a member of Group 2, you cannot assign deduction points, so your total cost in this stage is simply:

$$\text{total cost of deductions} = 3 \times \text{total received deduction points}$$

Keep in mind, your final payoff cannot be less than zero. Therefore, your total costs of deductions will never be higher than your initial payoff.

Members of Group 1, when you have finished making your decisions, click *Continue*. By clicking *Continue*, the experiment will go on to the next stage. You cannot change your decision after clicking *Continue*.

5. Final payoff (Group 1 & Group 2)

This is the fifth and last stage. Here you will view your final payoff.

If you are a member of Group 1, your will see **Final Payoff Group 1**. Your final payoff is your initial payoff minus your cost of assigned deductions and your loss from received deductions. Notice that this amount can never be less than zero. You will also see your total payoff for all periods. Please make note of these amounts on your Personal Record Table.

If you are a member of Group 2, your will see **Final Payoff Group 2**. If you were randomly selected for monitoring, your final payoff is your initial payoff minus your cost of received deductions. If you were not randomly selected for monitoring, your final payoff is the same as your initial payoff. Notice that this amount can never be less than zero. You will also see your total payoff for all periods. Please make note of these amounts on your Personal Record Table.

Click *Continue* when you are ready to go on.

After all participants have clicked *Continue*, the period will end, and you will go to the next period, where you will start over. Remember that your group assignment and ID stay the same in each period. After Period 15, the experiment will end, and you will be paid privately based on the conversion rate.

Summary

When the experiment begins, you will be randomly divided into groups of eight. Then, within your group, you will be randomly assigned to one of two subgroups: Group 1 or Group 2. You will be randomly assigned an ID number within your group. Your group assignment and ID assignment will be the same throughout the experiment.

You will participate in 15 periods of this experiment.

Members of Group 1 will participate in a communication stage in period 1, 3, 5, 7, 9, 11, 13, and 15.

All participants will receive an endowment of 12 tokens in each period. You will decide how much of your endowment to invest into The Account, which is shared by all participants. What you earn from The Account depends on what you invest and what everyone else invests. You will also earn \$2.00L for each token you choose not to invest and a fixed payoff of \$88.80L in each period.

Once your initial payoff has been determined you will move to the Deductions stage. In this stage members of Group 1 will be able to assign deduction points to the other members of Group 1 and one randomly selected member of Group 2. Each deduction point you assign costs \$1.00L and reduces the payoff of the person receiving the deduction point by \$3.00L. Keep in mind that the cost of deduction points you assign cannot exceed your initial payoff, and that the total cost of deduction points cannot reduce your payoff below zero.

At the end of each period you will be informed of your initial payoff, your cost of assigned deductions, your loss from received deductions, your total cost from deductions, your final payoff for the period and your total payoff for all of the periods which have been completed.

Questions

You are welcome to ask any questions you have about the experiment. If at any time you do have a question, or are unclear about something, raise your hand. Someone will come over to assist you.

If there are no questions at this time, we will begin the experiment.

Initial Payoff Table

Total Investment in The Account (# of tokens)	Total Payoff from The Account
0	0.00
4	117.60
8	222.40
12	314.40
16	393.60
20	460.00
24	513.60
28	554.40
32	582.40
36	597.60
40	600.00
44	589.60
48	566.40
52	530.40
56	481.60
60	420.00
64	345.60
68	258.40
72	158.40
76	45.60
80	-80.00
84	-218.40
88	-369.60
92	-533.60
96	-710.40

APPENDIX B

CHAPTER 2

Treatment differences of insider group account allocation and cooperation indices

Table B.1: Linear random effects regression models of treatment differences of insider group account allocation and cooperation indices.

	CPR		PG	
	(1) Group account Allocation	(2) Cooperation index	(3) Group account Allocation	(4) Cooperation index
Theft	-3.347***	0.067***	-1.178	-0.024
	(0.43)	(0.01)	(1.42)	(0.03)
Constant	36.169***	0.277***	17.386***	0.348***
	(0.32)	(0.01)	(1.19)	(0.02)
N	720	720	720	720
R-squared overall	0.020	0.020	0.002	0.002

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Experiment instructions

Note: the instructions to our control treatments were identical to those of Kingsley and Liu (2014). The only difference in the instructions to the theft treatments are Tables B.2 (CPR) and B.3 (PG) in Stage 2.

Instructions

Welcome to the experiment. Please follow along as these instructions are read. If you have a question please raise your hand. This information packet will explain the decision you will make and how your decision affects your individual earnings.

The experiment consists of 15 rounds. You will be randomly grouped together into groups of 6. You will then be randomly split between Group 1 (4 members) or Group 2 (2 members) and receive a corresponding group ID. Your group and group ID will remain the same throughout the experiment. At no point during this experiment will the other members of your group be known to you. You will be compensated, privately and in cash, at the end of the experiment.

Stage 1: Decision (Group 1)

At the beginning of each round you will each receive 50 Experimental Dollars (ED). The decision you are asked to make consists of allocating your 50 ED between two accounts. Specifically, you will be asked how many of your 50 ED you would like to invest in Account 1.

Account 1

You can choose to invest any whole number, X, into Account 1 between $0-50$. The payoffs you receive from Account 1 depend not only on the amount you invest, but also on the investment decisions of the other 3 members of your group, and the decision from Group 2. The formula for Account 1 payoffs accompanies Table 1 below. Note that the payoffs in this table do not include the decisions of Group 2.

Account 2

After choosing how many ED to invest in Account 1 your remaining ED will automatically be invested in Account 2. The payoffs you receive from Account 2 depend only on your investment. Each ED you invest in Account 2 gives you a payoff of 1 ED. Therefore, if you invest X ED in Account 1 you will invest $(50 - X)$ ED in Account 2 and this will give you a payoff of $(50 - X)$ ED from Account 2.

Group 1: Initial Individual Payoff

Your initial payoffs *per round* are the sum of your payoffs in Account 1 and your payoffs in Account 2. You can accumulate additional earnings each round. At the conclusion of the experiment your accumulated ED will be converted into cash such that 100 ED is worth $1.00.

Table B.2 (Table B.3) describes your **initial individual payoffs** where the row labeled **X** shows the different investment levels in Account 1 that **you** can choose (0 – 50 in steps of 5). The column labeled **Y** shows the different **sums of investment** in Account 1 that the other 3 members of your group may choose (0 – 150 in steps of 5). Table B.2 (Table B.3) shows the initial payoffs you earn if you choose to invest X and the sum of the investment of the others is Y. In other words, the entry corresponding to column **Y** and row **X** indicates your payoffs in case your investment into Account 1 is **X** and the sum of the investment of the others is Y. Note that the payoffs in this table do not include the decisions of Group 2.

CPR: Notice that for **many** levels of group investment (**Y**) an increase in your individual investment (**X**) **increases** your individual payoff. To demonstrate, choose a couple values for **Y** and consider your payoffs as **X** increases. However, for **any** level of individual investment ($\mathbf{X} > \mathbf{0}$) an increase in group investment (**Y**) **decreases** your individual payoff. To demonstrate, choose a couple values for **X** and consider your payoffs as **Y** increases. Spend a minute or two looking at Table B.2 and ask any questions you have.

Y/X	0	5	10	15	20	25	30	35	40	45	50
0	50.00	74.38	97.50	119.38	140.00	159.38	177.50	194.38	210.00	224.38	237.50
5	50.00	73.75	96.25	117.50	137.50	156.25	173.75	190.00	205.00	218.75	231.25
10	50.00	73.12	95.00	115.62	135.00	153.12	170.00	185.62	200.00	213.12	225.00
15	50.00	72.50	93.75	113.75	132.50	150.00	166.25	181.25	195.00	207.50	218.75
20	50.00	71.88	92.50	111.88	130.00	146.88	162.50	176.88	190.00	201.88	212.50
25	50.00	71.25	91.25	110.00	127.50	143.75	158.75	172.50	185.00	196.25	206.25
30	50.00	70.62	90.00	108.12	125.00	140.62	155.00	168.12	180.00	190.62	200.00
35	50.00	70.00	88.75	106.25	122.50	137.50	151.25	163.75	175.00	185.00	193.75
40	50.00	69.38	87.50	104.38	120.00	134.38	147.50	159.38	170.00	179.38	187.50
45	50.00	68.75	86.25	102.50	117.50	131.25	143.75	155.00	165.00	173.75	181.25
50	50.00	68.12	85.00	100.62	115.00	128.12	140.00	150.62	160.00	168.12	175.00
55	50.00	67.50	83.75	98.75	112.50	125.00	136.25	146.25	155.00	162.50	168.75
60	50.00	66.88	82.50	96.88	110.00	121.88	132.50	141.88	150.00	156.88	162.50
65	50.00	66.25	81.25	95.00	107.50	118.75	128.75	137.50	145.00	151.25	156.25
70	50.00	65.62	80.00	93.12	105.00	115.62	125.00	133.12	140.00	145.62	150.00
75	50.00	65.00	78.75	91.25	102.50	112.50	121.25	128.75	135.00	140.00	143.75
80	50.00	64.38	77.50	89.38	100.00	109.38	117.50	124.38	130.00	134.38	137.50
85	50.00	63.75	76.25	87.50	97.50	106.25	113.75	120.00	125.00	128.75	131.25
90	50.00	63.12	75.00	85.62	95.00	103.12	110.00	115.63	120.00	123.12	125.00
95	50.00	62.50	73.75	83.75	92.50	100.00	106.25	111.25	115.00	117.50	118.75
100	50.00	61.88	72.50	81.88	90.00	96.88	102.50	106.88	110.00	111.88	112.50
105	50.00	61.25	71.25	80.00	87.50	93.75	98.75	102.50	105.00	106.25	106.25
110	50.00	60.62	70.00	78.12	85.00	90.62	95.00	98.12	100.00	100.62	100.00
115	50.00	60.00	68.75	76.25	82.50	87.50	91.25	93.75	95.00	95.00	93.75
120	50.00	59.38	67.50	74.38	80.00	84.38	87.50	89.38	90.00	89.38	87.50
125	50.00	58.75	66.25	72.50	77.50	81.25	83.75	85.00	85.00	83.75	81.25
130	50.00	58.12	65.00	70.62	75.00	78.12	80.00	80.62	80.00	78.12	75.00
135	50.00	57.50	63.75	68.75	72.50	75.00	76.25	76.25	75.00	72.50	68.75
140	50.00	56.88	62.50	66.88	70.00	71.88	72.50	71.88	70.00	66.88	62.50
145	50.00	56.25	61.25	65.00	67.50	68.75	68.75	67.50	65.00	61.25	56.25
150	50.00	55.62	60.00	63.12	65.00	65.62	65.00	63.12	60.00	55.62	50.00

Table B.2: Total Individual Payoff $= \underbrace{\frac{X}{X+Y}\left[6(X+Y) - 0.025(X+Y)^2\right]}_{\text{Account 1 Payoff}} + \underbrace{(50-X)}_{\text{Account 2 Payoff}}$

PG: Notice that for **many** levels of group investment (**Y**) a **decrease** in your individual investment (**X**) **increases** your individual payoff. To demonstrate, choose a couple values for **Y** and consider your payoffs as **X** decreases. Similarly, for many levels of individual investment (**X**) an increase in group investment (**Y**) increases your individual payoff. To demonstrate, choose a couple values for **X** and consider your payoffs as **Y** increases. Spend a minute or two looking at Table B.3 and ask any questions you have.

Y/X	0	5	10	15	20	25	30	35	40	45	50
0	50.00	52.34	54.38	56.09	57.50	58.59	59.38	59.84	60.00	59.84	59.38
5	57.34	59.38	61.09	62.50	63.59	64.38	64.84	65.00	64.84	64.38	63.59
10	64.38	66.09	67.50	68.59	69.38	69.84	70.00	69.84	69.38	68.59	67.50
15	71.09	72.50	73.59	74.38	74.84	75.00	74.84	74.38	73.59	72.50	71.09
20	77.50	78.59	79.38	79.84	80.00	79.84	79.38	78.59	77.50	76.09	74.38
25	83.59	84.38	84.84	85.00	84.84	84.38	83.59	82.50	81.09	79.38	77.34
30	89.38	89.84	90.00	89.84	89.38	88.59	87.50	86.09	84.38	82.34	80.00
35	94.84	95.00	94.84	94.38	93.59	92.50	91.09	89.38	87.34	85.00	82.34
40	100.00	99.84	99.38	98.59	97.50	96.09	94.38	92.34	90.00	87.34	84.38
45	104.84	104.38	103.59	102.50	101.09	99.38	97.34	95.00	92.34	89.38	86.09
50	109.38	108.59	107.50	106.09	104.38	102.34	100.00	97.34	94.38	91.09	87.50
55	113.59	112.50	111.09	109.38	107.34	105.00	102.34	99.38	96.09	92.50	88.59
60	117.50	116.09	114.38	112.34	110.00	107.34	104.38	101.09	97.50	93.59	89.38
65	121.09	119.38	117.34	115.00	112.34	109.38	106.09	102.50	98.59	94.38	89.84
70	124.38	122.34	120.00	117.34	114.38	111.09	107.50	103.59	99.38	94.84	90.00
75	127.34	125.00	122.34	119.38	116.09	112.50	108.59	104.38	99.84	95.00	89.84
80	130.00	127.34	124.38	121.09	117.50	113.59	109.38	104.84	100.00	94.84	89.38
85	132.34	129.38	126.09	122.50	118.59	114.38	109.84	105.00	99.84	94.38	88.59
90	134.38	131.09	127.50	123.59	119.38	114.84	110.00	104.84	99.38	93.59	87.50
95	136.09	132.50	128.59	124.38	119.84	115.00	109.84	104.38	98.59	92.50	86.09
100	137.50	133.59	129.38	124.84	120.00	114.84	109.38	103.59	97.50	91.09	84.38
105	138.59	134.38	129.84	125.00	119.84	114.38	108.59	102.50	96.09	89.38	82.34
110	139.38	134.84	130.00	124.84	119.38	113.59	107.50	101.09	94.38	87.34	80.00
115	139.84	135.00	129.84	124.38	118.59	112.50	106.09	99.38	92.34	85.00	77.34
120	140.00	134.84	129.38	123.59	117.50	111.09	104.38	97.34	90.00	82.34	74.38
125	139.84	134.38	128.59	122.50	116.09	109.38	102.34	95.00	87.34	79.38	71.09
130	139.38	133.59	127.50	121.09	114.38	107.34	100.00	92.34	84.38	76.09	67.50
135	138.59	132.50	126.09	119.38	112.34	105.00	97.34	89.38	81.09	72.50	63.59
140	137.50	131.09	124.38	117.34	110.00	102.34	94.38	86.09	77.50	68.59	59.38
145	136.09	129.38	122.34	115.00	107.34	99.38	91.09	82.50	73.59	64.38	54.84
150	134.38	127.34	120.00	112.34	104.38	96.09	87.50	78.59	69.38	59.84	50.00

Table B.3: Total Individual Payoff $= \underbrace{0.25\left[6(X+Y) - 0.025(X+Y)^2\right]}_{\text{Account 1 Payoff}} + \underbrace{(50-X)}_{\text{Account 2 Payoff}}$

Initial Group Payoff

Table B.4 describes the **initial group payoff**. That is, *the sum of the initial individual payoffs for each member of the group.* Again, **X** represents your individual Account 1 investment, and **Y** represents the sum of Account 1 investment from the other three members of your group. Notice that initial group payoff increases until a total **(X+Y)** of **100 ED** are invested into Account 1 and decreases thereafter. Again, note that the payoffs in this table do not include the decisions of Group 2. Spend a minute or two looking at Table B.4 and ask any questions you have.

Y/X	0	5	10	15	20	25	30	35	40	45	50
0	200.00	224.38	247.50	269.38	290.00	309.38	327.50	344.38	360.00	374.38	387.50
5	224.38	247.50	269.38	290.00	309.38	327.50	344.38	360.00	374.38	387.50	399.38
10	247.50	269.38	290.00	309.38	327.50	344.38	360.00	374.38	387.50	399.38	410.00
15	269.38	290.00	309.38	327.50	344.38	360.00	374.38	387.50	399.38	410.00	419.38
20	290.00	309.38	327.50	344.38	360.00	374.38	387.50	399.38	410.00	419.38	427.50
25	309.38	327.50	344.38	360.00	374.38	387.50	399.38	410.00	419.38	427.50	434.38
30	327.50	344.38	360.00	374.38	387.50	399.38	410.00	419.38	427.50	434.38	440.00
35	344.38	360.00	374.38	387.50	399.38	410.00	419.38	427.50	434.38	440.00	444.38
40	360.00	374.38	387.50	399.38	410.00	419.38	427.50	434.38	440.00	444.38	447.50
45	374.38	387.50	399.38	410.00	419.38	427.50	434.38	440.00	444.38	447.50	449.38
50	387.50	399.38	410.00	419.38	427.50	434.38	440.00	444.38	447.50	449.38	450.00
55	399.38	410.00	419.38	427.50	434.38	440.00	444.38	447.50	449.38	450.00	449.38
60	410.00	419.38	427.50	434.38	440.00	444.38	447.50	449.38	450.00	449.38	447.50
65	419.38	427.50	434.38	440.00	444.38	447.50	449.38	450.00	449.38	447.50	444.38
70	427.50	434.38	440.00	444.38	447.50	449.38	450.00	449.38	447.50	444.38	440.00
75	434.38	440.00	444.38	447.50	449.38	450.00	449.38	447.50	444.38	440.00	434.38
80	440.00	444.38	447.50	449.38	450.00	449.38	447.50	444.38	440.00	434.38	427.50
85	444.38	447.50	449.38	450.00	449.38	447.50	444.38	440.00	434.38	427.50	419.38
90	447.50	449.38	450.00	449.38	447.50	444.38	440.00	434.38	427.50	419.38	410.00
95	449.38	450.00	449.38	447.50	444.38	440.00	434.38	427.50	419.38	410.00	399.38
100	450.00	449.38	447.50	444.38	440.00	434.38	427.50	419.38	410.00	399.38	387.50
105	449.38	447.50	444.38	440.00	434.38	427.50	419.38	410.00	399.38	387.50	374.38
110	447.50	444.38	440.00	434.38	427.50	419.38	410.00	399.38	387.50	374.38	360.00
115	444.38	440.00	434.38	427.50	419.38	410.00	399.38	387.50	374.38	360.00	344.38
120	440.00	434.38	427.50	419.38	410.00	399.38	387.50	374.38	360.00	344.38	327.50
125	434.38	427.50	419.38	410.00	399.38	387.50	374.38	360.00	344.38	327.50	309.38
130	427.50	419.38	410.00	399.38	387.50	374.38	360.00	344.38	327.50	309.38	290.00
135	419.38	410.00	399.38	387.50	374.38	360.00	344.38	327.50	309.38	290.00	269.38
140	410.00	399.38	387.50	374.38	360.00	344.38	327.50	309.38	290.00	269.38	247.50
145	399.38	387.50	374.38	360.00	344.38	327.50	309.38	290.00	269.38	247.50	224.38
150	387.50	374.38	360.00	344.38	327.50	309.38	290.00	269.38	247.50	224.38	200.00

Table B.4: Total Group 1 Payoff $= \underbrace{\left[6(X+Y) - 0.025(X+Y)^2\right]}_{\text{Account 1 Payoff}} + \underbrace{(200 - (X+Y))}_{\text{Account 2 Payoff}}$

Stage 2: Decision (Group 2)

At the beginning of each round you will each receive 50 Experimental Dollars (ED). The decision you are asked to make is how many of your 50 ED you would like to use to take from the payoff of Account 1.

Taking from Account 1

After members of Group 1 have made their decisions, you will see the initial payoff from Account 1. By investing some amount of your 50 ED, you can transfer payoffs from Account 1 to yourself. The formula for payoffs from Account 1 accompanies Table 3 below.

Account 2

After choosing how many ED to invest in a take from Account 1 your remaining ED will automatically be invested in Account 2. The payoffs you receive from Account 2 depend only on your investment. Each ED you invest in Account 2 gives you a payoff of 1 ED. Therefore, if you invest X ED in Account 1 you will invest $(50 - X)$ ED in Account 2 and this will give you a payoff of $(50 - X)$ ED from Account 2.

Group 2: Initial Individual Payoff

Your initial payoffs *per round* are the sum of your payoffs in Account 1 and your payoffs in Account 2. You can accumulate additional earnings each round. At the conclusion of the experiment your accumulated ED will be converted into cash such that 100 ED is worth $1.00.

Table B.5 describes your **initial individual payoffs** where the row labeled **X** shows the different levels of investment **you** choose to transfer the payoffs from Account 1 to yourself (0 – 50 in steps of 5). The column labeled **P** shows the different **payoffs** of Account 1 based on the decisions of Group 1. Table B.5 shows the initial payoffs you earn if you choose to invest **X** and the payoffs of Account 1 are **P.** In other words, the entry corresponding to column **P** and row **X** indicates your payoffs when the payoffs to Account 1 are **P** and you invest **X**.

Notice that for **many** levels of payoffs to Account 1 an increase in your take (**X**) increases your individual payoff. To demonstrate, choose a couple values for **P** and consider your payoffs as **X** increases. Spend a minute or two looking at Table 3 and ask any questions you have.

P/X	0	5	10	15	20	25	30	35	40	45	50
0	50.00	45.00	40.00	35.00	30.00	25.00	20.00	15.00	10.00	5.00	0.00
10	50.00	45.25	40.50	35.75	31.00	26.25	21.50	16.75	12.00	7.25	2.50
20	50.00	45.50	41.00	36.50	32.00	27.50	23.00	18.50	14.00	9.50	5.00
30	50.00	45.75	41.50	37.25	33.00	28.75	24.50	20.25	16.00	11.75	7.50
40	50.00	46.00	42.00	38.00	34.00	30.00	26.00	22.00	18.00	14.00	10.00
50	50.00	46.25	42.50	38.75	35.00	31.25	27.50	23.75	20.00	16.25	12.50
60	50.00	46.50	43.00	39.50	36.00	32.50	29.00	25.50	22.00	18.50	15.00
70	50.00	46.75	43.50	40.25	37.00	33.75	30.50	27.25	24.00	20.75	17.50
80	50.00	47.00	44.00	41.00	38.00	35.00	32.00	29.00	26.00	23.00	20.00
90	50.00	47.25	44.50	41.75	39.00	36.25	33.50	30.75	28.00	25.25	22.50
100	50.00	47.50	45.00	42.50	40.00	37.50	35.00	32.50	30.00	27.50	25.00
110	50.00	47.75	45.50	43.25	41.00	38.75	36.50	34.25	32.00	29.75	27.50
120	50.00	48.00	46.00	44.00	42.00	40.00	38.00	36.00	34.00	32.00	30.00
130	50.00	48.25	46.50	44.75	43.00	41.25	39.50	37.75	36.00	34.25	32.50
140	50.00	48.50	47.00	45.50	44.00	42.50	41.00	39.50	38.00	36.50	35.00
150	50.00	48.75	47.50	46.25	45.00	43.75	42.50	41.25	40.00	38.75	37.50
160	50.00	49.00	48.00	47.00	46.00	45.00	44.00	43.00	42.00	41.00	40.00
170	50.00	49.25	48.50	47.75	47.00	46.25	45.50	44.75	44.00	43.25	42.50
180	50.00	49.50	49.00	48.50	48.00	47.50	47.00	46.50	46.00	45.50	45.00
190	50.00	49.75	49.50	49.25	49.00	48.75	48.50	48.25	48.00	47.75	47.50
200	50.00	50.00	50.00	50.00	50.00	50.00	50.00	50.00	50.00	50.00	50.00
210	50.00	50.25	50.50	50.75	51.00	51.25	51.50	51.75	52.00	52.25	52.50
220	50.00	50.50	51.00	51.50	52.00	52.50	53.00	53.50	54.00	54.50	55.00
230	50.00	50.75	51.50	52.25	53.00	53.75	54.50	55.25	56.00	56.75	57.50
240	50.00	51.00	52.00	53.00	54.00	55.00	56.00	57.00	58.00	59.00	60.00
250	50.00	51.25	52.50	53.75	55.00	56.25	57.50	58.75	60.00	61.25	62.50
260	50.00	51.50	53.00	54.50	56.00	57.50	59.00	60.50	62.00	63.50	65.00
270	50.00	51.75	53.50	55.25	57.00	58.75	60.50	62.25	64.00	65.75	67.50
280	50.00	52.00	54.00	56.00	58.00	60.00	62.00	64.00	66.00	68.00	70.00
290	50.00	52.25	54.50	56.75	59.00	61.25	63.50	65.75	68.00	70.25	72.50
300	50.00	52.50	55.00	57.50	60.00	62.50	65.00	67.50	70.00	72.50	75.00
310	50.00	52.75	55.50	58.25	61.00	63.75	66.50	69.25	72.00	74.75	77.50
320	50.00	53.00	56.00	59.00	62.00	65.00	68.00	71.00	74.00	77.00	80.00
330	50.00	53.25	56.50	59.75	63.00	66.25	69.50	72.75	76.00	79.25	82.50
340	50.00	53.50	57.00	60.50	64.00	67.50	71.00	74.50	78.00	81.50	85.00
350	50.00	53.75	57.50	61.25	65.00	68.75	72.50	76.25	80.00	83.75	87.50
360	50.00	54.00	58.00	62.00	66.00	70.00	74.00	78.00	82.00	86.00	90.00

Table B.5: $\text{Total Individual Payoff} = \underbrace{(X)(0.01)(0.5)(P)}_{\text{Account 1 Take Payoff}} + \underbrace{(50-X)}_{\text{Account 2 Payoff}}$,
where $P = 6(\text{Total Group Investment}) - 0.025(\text{Total Group 1 Investment})^2$

Stage 3: Initial Payoffs (Group 1 & Group 2)

- **Group 1.** In this stage you will view: (1) your individual investment decision X, (2) the sum of all Account 1 investment (including yours) by the group $X+Y$, (3) the initial payoff to your group from Account 1, (4) the total amount of Account 1 taken by Group 2, (5) the total losses due to the decisions by members of Group 2, (6) your Account 1 payoff, (7) your Account 2 payoff and (8) your initial payoff (Account 1 + Account 2).

- **Group 2.** In this stage you will view: (1) your individual investment decision (your take from Account 1 payoffs), (2) your Account 1 payoff, (3) your Account 2 payoff and (4) your individual payoff (Account 1 + Account 2).

Stage 4: Deductions (Group 1 & Group 2)

In this stage, you can decide to decrease the payoff of Group 2 participants by assigning deduction points. Only members of Group 1 can assign deduction points. If you are a member of Group 1, you may enter a number of deduction points for each participant. If you do not wish to change the payoff of a participant, then you must enter 0.

Assigning deductions: Group 1

You will incur costs from assigning deduction points. Each deduction point you assign will cost you $1ED and cost the receiver $3ED.

For example, if you assign 2 deduction points to a Group 2 participant, this costs you $2ED and it costs the receiver $6ED. If you assign 4 deduction points the other Group 2 participant, this costs you an additional $4ED and it costs the receiver $12ED. In this example you will have assigned 6 deduction points, costing you $6ED.

To view the cost of your assigned deductions, click the button *Cost*. Your deduction assignment cost is calculated as:

$$\text{Total cost of assigned deductions} = 1 \times \text{Total assigned deduction points}$$

You can change your decision as long as you have not left the stage. To recalculate the costs after changing your assigned points, simply click *Cost* again.

Please note your cost of assigned deductions cannot exceed your initial payoff.

Receiving deductions: Group 2

If you receive a total of one deduction point, your payoff will be decreased by \$3ED. If you receive a total of 2 deduction points, your payoff will be decreased by \$6ED, and so on. Your loss from received deductions are calculated as:

$$\text{Total cost of received deductions} = 3 \times \text{Total received deduction points}$$

Please note, your cost of received deductions cannot exceed your initial payoff.

Stage 5: Total Payoffs (Group 1 & Group 2)

- **Group 1.** In this stage you will view: (1) your initial payoff, (2) your loss from deductions (based on the number of deduction points you assigned), (3) your total payoff for this period (initial payoff minus deductions) and (4) your accumulated earnings up to this point in the game.

- **Group 2.** In this stage you will view: (1) your initial payoff, (2) the total deduction points assigned to you by members of Group 1, (3) your loss from deductions, (4) your total payoff for this period (initial payoff minus deductions), and (4) your accumulated earnings up to this point in the game.

APPENDIX C

CHAPTER 3

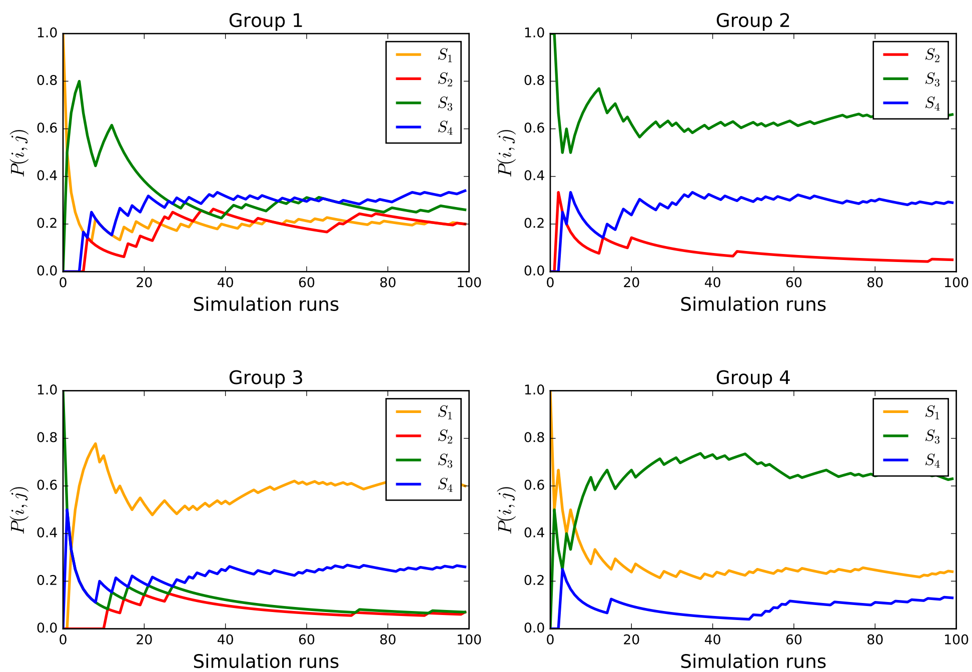

Figure C.1: Simulated Markov chains for enforcement networks in Partial Monitoring.

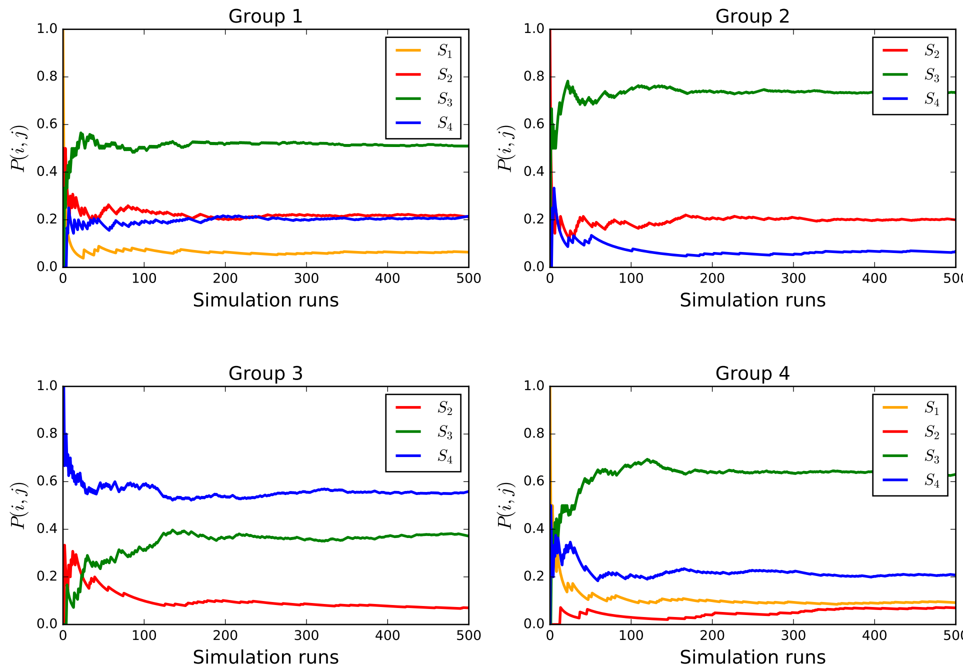

Figure C.2: Simulated Markov chains for enforcement networks in Full Monitoring.

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